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NOTICE.

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STELLAR SPECTRA.

[*Second Paper.*]

By E. W. MAUNDER, F.R.A.S. (*Assistant superintending the Spectroscopic Department of Greenwich Observatory*).

THE last paragraph of my paper on this subject in the April number of KNOWLEDGE was cut abruptly short, and would bear further explanation, and the Editor having courteously invited me to resume the subject, I accordingly do so.

The method of comparing the mass-brightness of double stars, which Mr. Monck devised (*Observatory*, vol. x., p. 96), depends upon the circumstance that the period of a binary is not affected by its distance from us, and though its mass will vary according to the distance from us which we assign to the star, its total brightness will vary in the same proportion. Given then for two pairs of double stars the apparent brightness, the apparent angular radii of the relative orbits of the two components, and the periods of each, and their relative mass-brightness—or to use Prof. Young's more graphic phraseology, their relative "candle power per ton"—can be computed. The formula only breaks down in the improbable case of a pair in which one star is at once very much fainter, and yet nearly as heavy as the other.

The following table shows the values of the relative mass-brightness of the stars of the two types as given by Mr. Gore in his "Catalogue of Binary Stars":—

SIRIAN STARS.

OΣ 4	1·48	ω Leonis	4·06	ξ Scorpii	5·70
OΣ 20	9·77	ϕ Ursæ Maj.	21·26	λ Ophiuchi	29·54
14 Orionis	4·09	γ Virginis	4·92	μ Draconis	4·21
12 Lyncis	14·01	25 Can. Ven.	7·28	ζ Sagittarii	23·04
Sirius	6·35	η Coronæ Bor.	1·40	δ Cygni	31·29
Castor	38·12	μ^3 Bootis	2·74	β Delphini	6·85
ζ Cancri	2·90	γ Coronæ Bor.	22·10	λ Cygni	11·01

SOLAR STARS.

Σ 3062	0·68	42 Comæ	2·46	Σ 2173	0·88
η Cassiopeiæ	0·33	Σ 1757	0·37	τ Ophiuchi	7·35
36 Andromedæ	6·23	Σ 1819	0·95	70 Ophiuchi	0·30
Σ 228	1·17	α Centauri	1·31	γ Coronæ Aust.	1·22
OΣ 149	1·07	ξ Bootis	0·34	OΣ 387	2·46
Σ 1037	1·83	44 Bootis	2·07	OΣ 400	3·07
Σ 3121	0·21	OΣ 298	0·45	4 Aquarii	3·64
ξ Ursæ Maj.*	1·00	σ Coronæ Bor.	1·77	τ Cygni	4·28
OΣ 234	1·80	ζ Herculis	4·67	π Cephei	11·07
OΣ 235	2·11	Σ 2107	1·74		

* Mr. Monck employed this binary as his unit of comparison.

The table shows two striking features. First, an *average* superiority of the Sirian over the Solar stars, far too marked and too frequent to be the effect of accident, for whilst the mean mass-brightness of the First Type stars is 12·00, that of the Second Type is not one-fifth so great, being only 2·30. Indeed, one-third of the Sirian stars are brighter per mass unit than the brightest Solar star, and one-half the Solar are fainter than the faintest Sirian. So that we cannot resist the conclusion that, in proportion to their light-giving power, stars resembling our own sun in spectrum are considerably heavier, and if we assume a uniform brilliancy for equal photospheric areas, much denser than those like Sirius or Vega. But when we take this conclusion in connection with the one already arrived at, that the total light-giving power of the average member of the Second Type is greater than that of the First, the superiority in mass of the Solar stars becomes yet further enforced. If it be legitimate to ascribe the title "giant suns" to any one of the classes into which we divide stars, as distinguished by their spectra, then undoubtedly it must be the Solar and not the Sirian stars which must be so designated.

But the second conclusion to be drawn from the table would show that spectrum alone, at least as at present classified, is insufficient to determine whether the mass-brightness of a star is relatively high or low. Just as with the table given on p. 73, stars of every degree of absolute light-giving power were found in either type, so it is now with mass-brightness. We could not then suppose that any given dimensions, or any given total-light radiation, were necessarily associated with a particular form of spectrum; and so now we are precluded from supposing that any given degree of condensation corresponds to the one type or the other.

This conclusion has a more important bearing on the question of the "Age of Stars" than the former; using the word "age," of course, not to signify actual length of

existence, but condition of development. For whilst the total light-giving power of a star will depend not merely on its "age," but also on its size, so that we might expect both "old" and "young" stars to show a great diversity of emissive power, it is not so with its conditions of condensation. On the current theory of gradual cooling and contraction the mass-brightness would vary directly with the "age," and we should expect, if spectrum type simply meant stage of development, that there would be no very wide range of difference of mass-brightness in any particular class, but that different types would be clearly marked off from each other by very different brilliancies per unit of mass. It should be as easy to assign its spectrum to each star in the table, from a consideration of the number deduced for it, as from actual scrutiny by the prism. That the fact is so different shows that we cannot accept such speculations as those of Vogel and Lockyer as holding good generally; spectrum type may mean "age" in a number of instances, but it cannot always do so.

So far as the spectrum is an indication of the stage of development attained it will undoubtedly be true, as I said in April, that "theorists are probably right when they have placed the Second Type stars as belonging to a later stage than the First"; but we must bear clearly in mind that we have no right to assert that all stars, or even the majority, pass through both stages.

One most notable exception to the general greater density of the Second Type stars must be mentioned. This star, γ Leonis, with a relative candle power per ton of 92·99, stands out so distinguished for its superiority of brilliancy over mass that I have not included it amongst the other Second Type stars in the table. I think we are warranted in treating this star, the co-efficient of which is half as large again as all the other 29 stars of its type put together, as altogether exceptional. Nevertheless, its inclusion with the other members of the type would not have materially altered the result. The average First Type star would still have shown a higher mass-brightness than the Second Type, whilst it would have become more, and not less, obvious that condensation alone was not a sufficient indication of spectrum.

It is possible that the explanation of this peculiarity of γ Leonis may be that the Second Type is a stage passed through twice, once during the period of increasing temperature, and later, after the First Type has been gone through, during the time of decline. At present, however, the star stands quite alone, and the only star which can reasonably be suggested as also showing a similar extraordinary relative brightness is Arcturus. If Arcturus emits 147 times as much light as Sirius, it seems easier to suppose that this corresponds to a greater emissive power per ton, than to take the reverse view and assign it say 500 or 600 times the mass of Sirius.

It would be an immense assistance to us in an enquiry of this nature if we knew the details of the spectra of both members of a large number of pairs. Unfortunately, there are special difficulties about the examination of the spectra of double stars, and, in consequence, it is a branch of study which has been left practically untouched. We have, however, many records of the *colours* of binaries, and, in default of direct spectroscopic observation, they may be of some service. Now these give us a most noteworthy result. Where the two components of a binary are equal, or nearly so, then both are nearly of the same colour likewise. Where one is much smaller than the other, then in the great majority of cases the larger is yellowish, the smaller blue. The blue star is *never* in such a case the larger. Thus Mr. Lockyer gives, in his "Meteoritic Hypothesis," tables of 120 binary stars, from Mr. Chambers' revised "Bedford Catalogue." Of these 120 stars, the colours of the companion are not given in 5 cases, leaving 115 to work with. Of the 115, 55 pairs show the same colour—nearly always white to yellow—and the mean difference in brightness is less than one magnitude, and never exceeds two. In 30 cases, the principal star is white to yellow, and the small star blue, and for this class the mean difference of brightness is two magnitudes; and in 21 other cases the same arrangement of colours prevails, but the mean difference of brightness is six magnitudes. Five pairs only show the large star white or yellow, and the small star red.

The natural tendency would be to interpret the yellow stars as being probably of the Solar Type, as many of them certainly are, and the blue or purple stars as strongly-marked specimens of the Sirian Type. Mr. Lockyer does so, and so does M. Rocques in a recent paper on the "Age and Colour of Stars." In one instance at least we know that this interpretation is a correct one, and though it is very likely that some of these small deeply-coloured stars have spectra of a type as yet unrecognised, yet the assumption that many are Sirian in character is an extremely probable one.

If so, four independent methods result in showing us the Solar stars as on the average larger than the Sirian. First, the Solar stars are represented in a higher proportion amongst the third and fourth magnitudes than amongst the sixth and seventh. Next, amongst stars of which we know the parallax, the Solar stars prove to have the highest total light-giving power. Thirdly, the mean mass relative to the light-giving power is much greater for Solar stars. And now we see that when the two components of a pair are nearly equal they agree in colour, but when unequal, then the smaller would appear to be of the Sirian, the larger of the Solar Type.

It seems to me that this curious subordination of the blue stars in binary systems points to the principal star having amassed to itself the bulk of the metallic constituents of the nebula from which the system sprang; or else, if we adopt the idea of fission recently put forward by Herr See, that the smaller body when thrown off consisted mainly of the lighter elements, the heavier remaining in the principal star. In other words, in these cases spectral type depends upon original chemical constitution, and not upon the stage of stellar development attained.

A further circumstance which looks in the same direction is the marked crowding of particular types in special regions. Prof. Pickering has pointed out that whilst the Second and Third Types are pretty evenly divided between the Galactic and non-Galactic regions, two-thirds of the First Type stars are found in the Milky Way, whilst the Fourth Type stars notoriously affect Birmingham's "Red Region," in and near Cygnus. Then again, the stars with spectra resembling those discovered by Wolf and Rayet, *all*, though they are 33 in number, lie within 10° of the Galactic equator, the great majority within 2°. The Orion stars—except Betelgeuse—again have a type of spectrum of their own, scarcely found outside the limits of the constellation. It seems only natural to infer that these remarkable instances of unequal distribution of stars of particular types are due not to the prevalence of one particular evolutionary stage in certain districts, but to a similarity in actual composition.

Nevertheless, it will not do to entirely discard the idea of successive stages, as evidenced by the different spectral types. The table on previous page shows this. If type had never anything to do with age, it is hard to see why there should be such a marked superiority in mass-bright-

ness of Sirian over Solar stars. Then again, the complete gradation observed, not a shade of difference missing, from Sirius down to stars like Mira Ceti, or α Herculis, is a strong argument in favour of an historic as well as a chemical interpretation of spectral peculiarities. But I would urge that this supposed course of evolution need not be universally operative, it is not necessary that every star pass through every stage. It is, perhaps, not necessary that any one star passes through all. But just as we may suppose that in a typical star, successive conditions of temperature and condensation bring up into the "reversing layer" at one epoch hydrogen, at another magnesium, at a third the substances, whatsoever they be, that cause the beautiful bands which Mira shows; so in the wider evolution of a multiple system, or wider still, of the entire Galaxy, certain stars, certain districts may become richer than the rest in this or that element, to be poorer in others; so that they have, as it were, a special stage indelibly imprinted on them. Thus they may present the appearance of a given epoch without ever having passed through the one theoretically considered as preceding it, and without the possibility of their ever entering on the phase supposed to be the next.

To sum up. The Types of Stellar Spectra are not to be explained along one line only, but along two; they do not always denote the phases attained by the stars in their development; they often indicate instead, radical differences of constitution. But when they have the first, the historical significance, the Solar usually denotes a later epoch than the Sirian; possible exceptions being seen in the cases of γ Leonis and Arcturus. And on the average the Solar stars and not the Sirian are the brightest, and have the greatest masses, though they are inferior to the latter as to relative brightness per unit of mass.

THE ORIGIN OF THE CHALK.

By JOHN T. KEMP, M.A.Cantab.

THE origin of the Chalk is a question which, though it has been long under discussion, is not by any means satisfactorily settled. The early investigators were much puzzled through being unable to find any rock at all resembling it in process of formation at the present time. Its extreme freedom from clayey or sandy material led them to regard it as a deep-sea deposit; and, hence, upon the discovery of the Grey Ooze of the Atlantic Ocean, it was concluded, somewhat hastily perhaps, that it must be the missing analogue. Some writers even went so far as to maintain that we were still living in the Chalk age. The argument was supported not only by the occurrence of similar species of foraminifera and other lowly organisms in the two deposits, but by certain resemblances in their higher fauna. Sponges which abound in the Ooze are closely allied to the siphonias and ventriculites of the Chalk. Echinoderms are common in both. *Echinothuria*, a genus characterized by the possession of a flexible covering, and peculiar to the Chalk, finds its nearest living representatives in the deep-water forms, *Asthenosoma* and *Phormosoma*. A species of *Beryx*, a well-known genus of fishes from the Chalk, has also been found in the Ooze.

That the Chalk was not formed under precisely the same conditions as the Atlantic Ooze appears now to be quite certain, but whether the differences are sufficient entirely to upset the older theory is still a matter of dispute.

One of these differences is in the chemical composition of the two deposits. Whereas the Chalk is composed of *98 or 99 per cent. of carbonate of lime with a little silica and alumina, the Ooze only contains from 44 to 80 per cent. of carbonate of lime, with 5 to 8 per cent. of silica, and from 8 to 33 per cent. of alumina. In these analyses it must, however, be remembered that in the Ooze every constituent is taken into account, while in the Chalk no notice is taken of the flints which contain the greater part of the silica. Prof. Prestwich estimates them at from 4 to 6 per cent. of the whole mass. The difference in the average amount of silica, if any, is, however, so slight that, taken by itself, it does not militate against the similarity as to origin of the two deposits.

The high proportion of alumina in the Ooze, as compared with the Chalk, presents at first sight a greater difficulty to the acceptance of the older theory. Its abundance in the former deposit is undoubtedly due to mechanical causes, for only the slightest traces of it are present in solution in sea-water, and it is never secreted by living organisms, as silica and carbonate of lime are. This difference in composition is nevertheless compatible with close similarity of origin. For, supposing that other appearances all pointed in that direction, this one exception might easily be explained on several hypotheses, such as the subsidence of suspended material from the land before reaching the Chalk area owing to great distance or slowness of currents; or secular variations in the amount of meteoric, volcanic, or other kinds of dust falling upon the sea.

It is when we come to examine the stratigraphy of the Chalk more carefully, and to compare its fauna more closely with the fauna of the Atlantic Ooze, that we find the most cogent reasons for doubting their likeness of origin.

The White Chalk, as distinguished from other Cretaceous deposits of the same age, is confined to an area stretching from Britain and Northern France through Holland, Northern Germany, Southern Sweden, and Southern Russia to the Crimea and the Ural Mountains. On the older rocks around this region beach-lines have been discovered at various levels, while throughout the Chalk eroded surfaces are frequently met with, accompanied by changes of life. One of these breaks, or unconformities, occurs at the base of the zone† of *Micraster cor-testudinarium*, another separates that zone from the zone of *Micraster cor-anguinum*, while a third precedes the incoming of the Danian stage. There are also various others. The nearness of the coasts and the frequency with which the sea-bed was brought within the sphere of denuding action by earth movements are strongly indicative of shallow water.

The palæontological evidence on the whole strongly confirms this view, notwithstanding, as has already been stated, some apparently adverse facts.

The late Dr. Gwyn Jeffries finds that the seventy-one genera of Mollusca known in the Chalk are all forms whose closest existing allies inhabit comparatively shallow water, while the genera characterizing the Ooze are very rare or altogether wanting in the older formation. Brachiopoda and Cephalopoda, so abundant in the Chalk, have been shown by the "Challenger" dredgings to be very uncommon in the Ooze. Although Echinodermata are frequent, they are (like the Mollusca) mostly of small size, and but distantly related to the Cretaceous forms.

The Atlantic Ooze is evidently of exceedingly slow growth, while the Chalk, on the contrary, appears, from the

* This applies to the pure white Chalk only; the Chalk marl contains a much larger percentage of other matter.

† These "zones" are vertical sub-divisions of the Chalk characterized by the presence of particular species of fossils which are rarely, if ever, found at higher or lower levels. *Micraster* is a genus of sea-urchins.

XUM

state in which many of its remains are preserved, to have accumulated with considerable rapidity. Fishes are found with their scales undisturbed, cidarids with their spines *in sitû* and so forth, showing that they were entombed in material solid enough to support them before the destruction of their soft parts was accomplished. There is also reason to think, from the absolutely amorphous state of much of the calcareous matter, that some of it was of chemical or mechanical origin; that is, it is either a precipitate from solution or else detritus from pre-existing limestones. Dr. Sorby considers it impossible for calcite shells to form such a substance by their destruction, though aragonite ones may do so.

Such are the principal arguments which have been adduced in denial of the theory that the Chalk was an earlier equivalent of the Atlantic Ooze. If, as geologists are becoming more inclined to hold every day, that theory is untenable, what shall we put in its place?

According to Dr. A. R. Wallace, conditions almost identical with those under which the Chalk was formed exist at the present time in the northern part of the Gulf of Mexico. The researches of Pourtales show that the ocean bed is there covered with a fine white mud, closely resembling Chalk in composition, and which consists chiefly of the impalpable detritus of the coral-reefs which fringe the numerous islands, together with the skeletons of the foraminifera abounding in the warm waters of the region. Coral-reefs exist in the Chalk of Maastricht and Faroe, but with these exceptions they are almost unknown in the formation.

Prof. Prestwich also discusses the subject at considerable length in his "Geology." He thinks that much further investigation will be required in order to set the question at rest. In the meantime, he is of opinion that the Chalk was formed under conditions which have passed away, or at any rate are nowhere exactly realised at the present time. The stratigraphy indicates that it was deposited in a nearly enclosed sea of no great depth. The rivers flowing into this sea brought down a very exceptional amount of soluble silica, though not so much as in somewhat earlier times: some of the Upper Green-sand beds contain as much as 72 per cent. In the presence of solid silica (of which sponge spicules, &c., are made) or of organic matter, this substance was precipitated, and formed the flints which so often enclose remains of sponges, or occupy the tests of echinoderms.

WHAT IS THE CAUSE OF VOLCANIC ACTION?

By REV. H. N. HUTCHINSON, B.A., F.G.S.

IN our previous paper we endeavoured to explain the structure of a Volcano, and described briefly the chief phenomena of an eruption. "Is it possible," the reader may ask, "to form any conclusions as to how volcanic eruptions are brought about?"—in other words, to find out what is going on underneath, and so to obtain some idea of the cause or causes of these strange manifestations of subterranean activity. It must be confessed at once that, in the present state of scientific knowledge, no full and complete explanation is possible. Geologists and others are as yet but feeling their way cautiously towards the light which, perhaps before long, will illumine the dark recesses of this mysterious subject; but nevertheless, since volcanic action was first carefully studied by Mr. Scrope, Sir Charles Lyell, and others, such valuable material has been collected, that we are getting much nearer to a true theory now that the ground has been somewhat cleared. Others, among living geologists, have carried on researches of very great value, and so have thrown valuable light on the subject. It will, perhaps, hardly be necessary to point out that the main difficulty is our ignorance of the interior of the earth. If we could penetrate subterranean regions to a sufficient depth, and find out the physical conditions prevailing far below the surface, there would be little difficulty in finding out how Volcanos are worked. But since direct knowledge is impossible, the problem must be attacked indirectly. We are somewhat in the position of a medical man diagnosing a difficult case; only medical science has the great advantage of knowing accurately the internal structure of the human body. The earth, unfortunately, is a body the internal anatomy of which is unknown. Of its epidermis, or skin, we have learned a good deal, but beyond that all is speculation. Looking upon volcanic action as a curious disease from which our patient the world occasionally suffers, it may not be unprofitable to see if some rough sort of diagnosis of the case is possible.

For this purpose it will be necessary to consider volcanic action a little more generally. We must not confine our attention to any one outbreak of the disease, or to any one Volcano, but look at the subject as a whole, putting ourselves, as it were, in the position of the physician who judges of any one "case" from the experience he has derived from the study of a great many "cases."

Now the first thing to remark is that volcanic action goes through phases, of which there are three. First, there is the state of permanent eruption—this is not a dangerous state, because the steam keeps escaping all the time the safety-valve is working, and all goes on smoothly. The second state is one of moderate activity, with more or less violent eruptions at brief intervals—this is rather dangerous; the safety-valve is at times jammed. And thirdly, we have paroxysms of intense energy alternating with long periods of repose, sometimes lasting for centuries. These eruptions are extremely violent and cause widespread destruction; the safety-valve has got jammed and so the boiler bursts. No Volcano has been so carefully watched for a long time as Vesuvius. Its history illustrates the phases we have just mentioned. The first recorded eruption is that of A.D. 73, a very severe one of the paroxysmal type, by which the towns of Herculaneum, Pompeii, and Stabiæ were buried up. We have an interesting account by the younger Pliny, whose uncle lost his life through remaining too near the scene of action, partly for the sake of rescuing those in danger and partly because he wished to observe the strange phenomenon. Before this great eruption took place Vesuvius had been in a quiescent state for 800 years, and if we may judge from Greek and Roman writings was not even suspected of latent possibilities in the way of volcanic eruptions. Then followed an interval of rest till the reign of Severus, the second eruption taking place in the year 203. In the year 472, says Procopinus, all Europe was covered, more or less, with volcanic ashes. Other eruptions followed at intervals, but there was complete repose for two centuries, *i.e.* until the year 1306. In 1500 it was again active, then quiet again for 130 years. In 1631 there came another paroxysmal outburst. After this many eruptions followed, and they have been frequent ever since. Vesuvius is, therefore, now in the second stage of moderate activity.

But geologists can take a wider view of Volcanos than this; their researches into the volcanic action of remote geological periods have yielded important results, which may be briefly indicated here. They can sum up the history of a volcanic region, and the result seems to be somewhat as follows:—There is a regular cycle of changes;

the invasion of any particular area of the earth's surface by the volcanic forces is heralded by subterranean shocks causing earthquakes. A little later on, fissures are formed, as indicated by the rise of saline and thermal springs, and the issuing of carbonic acid and other gases at the surface of the earth. As the subterranean activity becomes more marked, the temperature of the springs and emitted gases increases, and at last a visible rent is formed, exposing highly heated and incandescent rocky matter below. From the fissure thus formed, the gas and vapours imprisoned in the incandescent rocks escape with such violence as to disperse the latter in the form of scoriæ and volcanic ash, or to cause them to pour out in streams or lava flows. The action generally becomes concentrated at one or several points along the line of action—that is, the line of fissure and dislocation. In this way, a chain of Volcanos is formed, which may become the seats of volcanic action for a long time. When the volcanic energies are no longer able to raise up the fluid materials so that they shall flow out of the cones which have been built up, nor to rend their sides and form parasitic cones, fissures with small cones may be formed in the plains around the great central Volcanos. Later on, as the heated rocky matter below cools down, the fissures become sealed up by consolidating lava, and the Volcanos fall into a condition of quiescence, after which they begin to suffer from the effects of exposure to the atmospheric agencies of decay, and thus become more or less worn away or "denuded." But still the existence of heated rocky matter at no great depth below is indicated by outbursts of gases and vapours, the formation of geysers, mud-volcanoes and ordinary thermal springs; gradually, however, even these manifestations become more feeble, and thus all visible signs of volcanic energy die away in the district. Such a cycle of changes may require millions of years, but by the study of Volcanos in every stage of their growth and decline, it is possible to reconstruct this outline of their life history.

That Volcanos are built up along lines of fissure in the earth's crust does not admit of any doubt. The present distribution of Volcanos over the earth is a striking proof of this, and, moreover, we have further evidence derived from the study of old volcanic areas, which have been, as it were, dissected and so brought to light by long-continued erosion or denudation. Let us look a little more closely at the present distribution of Volcanos on the earth's surface, for it reveals some interesting facts which must be borne in mind in forming any conclusions with regard to the possible cause of volcanic action. One rule we have already observed, viz., that Volcanos are mostly distributed along lines. Secondly, they seem to follow or coincide with great mountain chains, such as the Andes, Rocky Mountains, or the ranges of Central America. Thirdly, there is some kind of connection between Volcanos and the coast lines of continents. Fourthly, they are always near some body of water (*i.e.*, when in the active stage). Fifthly, they are situated in regions of the earth which are undergoing slow upheaval, and are absent from regions where subsidence is taking place.

In framing any conclusions with regard to the problem under consideration, we must remember that volcanic action depends mainly on two things—(1) a high temperature below the region of activity, (2) the presence of steam at a high pressure.

Superheated steam evidently plays a very important part, and the force which raises masses of molten lava to the surface may be that due to the expansive power of steam. Volcanic eruptions, then, are essentially gigantic explosions, such as are faintly imitated in the bursting of steam boilers. This is good as far as it goes, but we cannot take it as an explanation of volcanic action; for we require to know the source of the steam, and of the lava, as well as the reason for the high temperature necessary for the production of both. Where does the heat come from? and what is the source of so much steam? Sir Humphry Davy, discoverer of the metals of the alkalies and alkaline earths at the commencement of the present century, showed that the metals potassium and sodium when touched by water develop a great deal of heat; in fact they burn on water, decomposing it and uniting with the oxygen. This led him to throw out the idea that if pure metals exist far down in the earth's interior, the access of water and air might give rise by oxidation to a large amount of heat, sufficient in fact to produce volcanic phenomena. But later on he confessed that this chemical theory of Volcanos was unsatisfactory. If it were true, enormous quantities of hydrogen gas would necessarily be emitted during volcanic action, but this is not the case.

It will readily be perceived that all explanations of volcanic action resolve themselves finally into the question of the condition of the earth's interior, with regard to which we can at present only speculate; hence the absence of any complete and consistent explanation of the volcanic problem.

Certain facts undoubtedly tend to establish the idea, once firmly maintained, that the whole of the earth's interior is in a highly heated state, but they do not prove it. The well-known increase of temperature as we descend mines, which is about 1° F. for every 50 or 60 feet, is not sufficient proof, for the *rate of increase* does not seem to be maintained as we descend to the greatest depths, and it is possible that the centre may be cold.* Still, astronomers tell us that the earth has been for ages a cooling globe, so that it would seem natural to suppose that there are still vast stores of heat within; but they may be more or less local.

It has even been argued, at one time, that the whole interior of the earth is in a molten condition, with only a thin crust of solid matter forming a kind of shell or outer

* It is conceivable, though it is not probable, that the central portions might not be warmer than the regions which have been already explored; but it is impossible that, after the lapse of ages, they should remain cooler than the exterior layers. The sun was formerly supposed by Bode, Sir John Herschel, and other distinguished astronomers to be a cool body surrounded by two atmospheres, the inner one a partially opaque heat-absorbing atmosphere, and the outer *photosphere* a brilliant heat-giving atmosphere. If it had not been for the great desire which mankind has always had to suppose that other bodies are inhabited by beings similar to ourselves, such an idea would probably never have been entertained. Sir Isaac Newton considered and rejected the theory that the sun might have a cool body and yet be surrounded by a hot atmosphere. But his reasons were only given in a letter, and were probably not generally known till the middle of this century. They are so clearly expressed that they could not fail to have convinced any intelligent thinker. The letter of Sir Isaac Newton I refer to was published by Sir David Brewster in his Life of Newton, Vol. II., p. 455. Sir Isaac says, "Though the inward part of the sun were an earthy gross substance, yet if the liquid shining substance Mr. Flamsteed supposes to swim upon it be then hot, it will heat the matter within it as certainly as melted lead would heat an iron bullet immersed in it. Nor is it material whether the liquid matter on the sun be of any considerable thickness. An iron bullet would heat as fast in a quart as in an ocean of melted lead, this difference only excepted, that the bullet would cool a small quantity of lead more than a great one. If, then, the liquid matter swimming on the sun be but so thick as not to be cooled by the central (as it must be), it will certainly heat the central parts, for it imparts heat to the contiguous parts as fast as if it were thicker, by which means the central parts must become as hot as if the hot fluid matter surrounding it equalled the whole vortex. The whole body of the sun, therefore, must be red hot, and consequently void of magnetism, unless we suppose its magnetism of another kind from any we have."—A. C. RANYARD.

coating; but this doctrine of a thin crust has been abandoned on account of strong evidence telling against it. It is quite possible for the earth's interior to be highly heated and yet to remain in the solid state. The enormous pressure to which rocks lying below the earth's crust (a word still retained for convenience sake by geologists, but without at all implying a fluid state below) must be subjected may be quite sufficient to prevent their fusion, for pressure probably retards the melting of solid bodies as it does the converting into vapour of liquids. Sir William Thomson proved that if any large portion of the earth were liquid, or viscid, or even as elastic as a sphere of steel, it would rise and fall as the ocean does under the influence of the sun and moon, and thus produce tides which would considerably interfere with the tide of the ocean as we now observe it; for it is clear that, if the land rose and fell at the same time as the ocean, the one would obscure the other. There is, possibly, a slight tide in the earth, but only such as would be caused in a very non-elastic body; whereas with a fluid interior the crust would rise up and down considerably, and would partly hide or very considerably interfere with the tide we perceive in the ocean. Some geologists maintain that there may be a great solid nucleus, with a thin liquid layer over it, and a solid outer layer or crust; but there are some difficulties in the way of this theory which cannot be considered here.

Let us assume, however, that the lower regions of the earth are in a highly heated state. Calculating from the observed rate of increase of temperature on descending, it is concluded that at a depth of 50 miles below the surface there may be a temperature of 2000° C.—sufficient to melt even platinum (at the surface); and, further, let us suppose such a temperature to be due to the earth having once been in a molten condition (according to the nebular theory). We have got, then, our source of heat; let us see what use we can make of it. Some geologists would explain volcanic manifestations in this way. Vast quantities of sedimentary matter slowly accumulate on the floors of seas, in time forming great deposits of stratified rock, thousands of feet thick; as the materials accumulate the bed of the sea slowly sinks under the weight of all this material. The accumulation of so much rocky matter checks the flow of the earth's heat, and so raises the temperature of that region, thus acting as a bandage does on the human body. Such sedimentary deposits necessarily contain much water—probably as much as from 5 to 20 per cent. of their mass. As the strata sink, and as the earth's heat rises up to them, their imprisoned water may be raised to a temperature far above its ordinary boiling point. Streams of molten rock penetrate their mass, and generate more steam, or at least highly heated water, and in this way possibly a large amount of the sedimentary material might be melted into lava, and such lava, with its occluded steam, would be sure to find its way up through any lines of weakness in the overlying rocks (such as fissures, faults, &c.), and on reaching the surface would give rise to volcanic action. There is a good deal to be said in favour of this explanation, for it accounts for the steam which plays so important a part. But it leaves out of sight some other aspects of the question. Others accounted for the steam by the observed proximity of active Volcanos to bodies of water. They argued that the water penetrating by infiltration through the floor of the sea, found its way to highly heated masses of rock, and was converted into steam, which would find its way up through any fissures which the stratified rocks above might contain. This theory seems to harmonize well with some of the facts, but after what has been said it will not be necessary to go out of our way to account for the presence of the steam. Some geologists are inclined to believe that here and there, below the earth's surface, there may be local reservoirs of still molten matter, and that these are the supplies from which the lava flows come, but this idea fails to account for the association of Volcanos with mountain chains, and with seas, as well as other important facts. A much more promising explanation is that there are below the crust of the earth large masses of highly heated rock, *kept solid* by the enormous pressure of overlying rocks; but that when earth-movements taking place within the crust, such as the upheaving of mountain chains, take off some of the weight, the balance of pressure is no longer maintained, and so these highly heated rock masses run off into the liquid state, and find their way to the surface, producing volcanic action. There is much to be said in favour of this view. It rightly connects volcanic action with movements of upheaval, with mountain chains, and lines of weakness in the earth's crust.

Now, the rocks in mountain chains are found to have suffered considerably from the effects of heat and pressure, and are seen to be considerably altered from their original state. Shales and sandstones have been converted into slates and quartzites, or even into schists and gneiss, and in some cases into granite; so that metamorphism, as it is called, seems to be closely connected with the upheaval of mountain chains, the crumpling and folding of rocks, which is very conspicuous in mountains, and with outbursts of volcanic energy.

The late Mr. Louis Mallet put forward a very ingenious speculation which seemed to explain all these coincidences. He endeavoured to show that the earth's supposed internal heat might be dispensed with altogether, and that the earth-movements themselves would be able to produce all the heat required for volcanic action. Friction, we know, produces heat, and he argued that the enormous friction involved in the crumpling, crushing, folding, and displacing of large masses of rock would be sufficient to bring about the melting of portions here and there, and that volcanic action would follow wherever these reached the surface. The theory has attracted much attention, and was very ably worked out; but his experiments on the amount of heat developed by the crushing of small portions of rock were not quite satisfactory. That some amount of metamorphism is due to the heat produced by friction, and the crushing of rock during earth-movements may be readily granted, but it would be rash to assume more than this. We do not yet know at what rate these Titanic forces of mountain building work, and so any speculations as to the temperatures developed by friction must be rather vague. The theory, however, did good service in attempting to explain the close connection between phenomena which had previously been looked upon as in no way connected with each other. Its simplicity and comprehensiveness were much in its favour.

But it seems like going out of our way to find a source of heat. If the earth is still a cooling body, we have only to fall back upon its own supply of heat, and to suppose that somewhere not very far below the rocks forming its surface there is a zone of highly heated rock, possibly in a viscous state owing to pressure. As the earth slowly loses heat, contraction of the outer shell follows, and this must involve tangential strains and upheaving along certain lines, because it has to settle down on to a smaller surface. Thus continents are slowly raised, and mountain chains upheaved. These movements cause changes in pressure, and we can readily conceive that a change of pressure may bring about the displacement and wedging up of matter from below, which gets more and more fluid as it ascends

to regions where the pressure is less, until, on reaching the surface, it runs out as lava, and allows its occluded steam freely to escape.

ASTRONOMY AS TAUGHT BY ACADEMY PICTURES.

MR. EYRE CROWE'S picture in this year's Royal Academy Exhibition, representing Jeremiah Horrocks arriving in haste from his clerical duties to find Venus on the Sun, is one of those useful pictures which draw attention to a great man who has not been sufficiently known and appreciated, and I therefore criticize it in no carping spirit. The picture has already caught the public taste, and reproductions of it have appeared in several illustrated papers. I am no advocate of too close astronomical criticism of a picture which is not intended to teach Astronomy, but in this case Mr. Eyre Crowe evidently does intend to present an object lesson in astronomy, and he should either have devoted more time to Horrocks' own account of his observation, or he should have called at the Astronomical Society and applied to the obliging Librarian for advice as to whom to look to for help.

Mr. Eyre Crowe has drawn Venus far too small, and the beam of light should not fill the eyepiece of the telescope. There is no authority for the curious Equatorial mounting which he has drawn, or for the attachment of the sun-screen to the instrument by a rod; in fact, we know that such a mounting could not have been used. Horrocks had been examining the Sun all day, and through his narrow windows he could not have followed the Sun with such an instrument for more than half an hour at most. He would continually have been obliged to move and readjust the position of the Polar axis of his stand, a loss of time and opportunity which the ingenious Horrocks would never have risked. It is evident that the telescope was attached to the shutter, or was on a stand immediately behind a hole in the shutter. In the *Venus in Sole Visa*, in which Horrocks gives an account of his observation, he says: "When the time of observation approached, I retired to my apartment, and having closed the windows against the light, I directed my telescope—previously adjusted to a focus—through the aperture towards the Sun, and received his rays at right angles upon the paper already mentioned."

This paper had upon it a six inch circle, carefully divided into degrees, and a divided scale, which enabled him to measure the diameter of the body of Venus, and the distance of the planet from the Sun's limb. Horrocks goes on:

"The Sun's image exactly filled the circle, and I watched carefully and unceasingly for any dark body which might enter on the disc of light. . . . Anxiously intent, therefore, on the undertaking, through the greater part of the 23rd, and the whole of the 24th, I omitted no available opportunity of observing her ingress. I watched carefully on the 24th from sunrise to nine o'clock, and from a little before ten until noon, and at one in the afternoon, being called away in the intervals by business of the highest importance, which, for these ornamental pursuits, I could not with propriety neglect."

The place of Venus upon the Sun's disc is not quite low enough on the right-hand limb in Mr. Crowe's picture. Horrocks says that it was about "62° 30′," certainly between 60° and 65° from the top of the Sun's disc towards the right. The black disc representing Venus should be larger; Horrocks estimated it to be about a quarter of an inch in diameter or a little more, for he thought that Venus was more than 1′ 12″ and less than 1′ 30″ in diameter. The size of the telescope drawn by Mr. Crowe seems to be about right. The tube was evidently home-made. Horrocks says that he gave 2s. 6d. for the object glass. It is evident from the description of his observation, that Horrocks used what was then known as the Galilean form of telescope, that is, it had a negative and not a positive eyepiece, so that Mr. Crowe is right in drawing the emergent rays of light as not crossing after they issued from the eyepiece; but they should issue in a narrower cone; about a third of an inch in diameter near to the eyepiece. One cannot speak more accurately without knowing the negative focus of the lens used for an eyepiece.

Mr. Crowe should, I think, have drawn Horrocks in a full cassock, as he is returning from service; at this date the clergy wore them at all times. The face seems to me to be rather old for a man who could not have been more than 22. As to the age of Horrocks, see a paper in the *Astronomical Register* for December, 1874.

It is evident that Horrocks could not have entered the room and found the Sun upon his screen and Venus on its disc, for he had no clockwork to drive his telescope. He must have entered the darkened room, found the Sun, and carried his screen to the distance where the Sun's image would fill his divided circle. The picture would be more accurate as depicting Horrocks' friend Crabtree than Horrocks himself. For Crabtree did not measure the diameter of Venus and its place upon the Sun's disc, a task to which Horrocks, in spite of his excitement, set himself at once, and succeeded admirably. All interested in spreading an interest in Astronomy must, however, be thankful to Mr. Crowe, and I hope that he will have many imitators who will dare to run the gauntlet of criticism.

A. C. RANYARD.

THE TRAVELS AND LIFE-HISTORY OF A FUNGUS.

BY J. PENTLAND SMITH, M.A., B.Sc., &c., Lecturer on Botany, Horticultural College, Swanley.

WERE one to put the question "What is a fungus?" to a non-botanical observer, he would probably receive for an answer "A mushroom is a fungus," getting an example of the group instead of a definition.

A mushroom is a fungus, and a readily observed member of the group; but the botanical tyro quickly learns that this plant cannot afford him a typical example of the life-history of fungi, and that all do not make themselves so evident to the sight as this particular fungus; in fact, he would soon perceive that the air is loaded with the spores or reproductive cells of fungi which are ready to germinate whenever they find suitable quarters.

Within late years our knowledge of the lower members of the vegetable kingdom has made enormous strides, and results have been obtained by careful and prolonged study which would astound the botanists of fifty years ago were they now to appear upon the scene. Then it was believed that Fungi and Bacteria were produced by the decay of other living organisms, and that they were a *lusus naturæ*, a freak of Nature. Their sudden appearance on a spot where no previous indication of their presence had been manifested was adduced as an argument in favour of the doctrine of spontaneous generation, or *generatio æquivoca*, that from unorganized matter living beings may arise. In later times Prof. Tyndall was an upholder of this theory; but as the outcome of his numerous and carefully

elaborated experiments he was forced to confess that no such thing as spontaneous generation takes place—at least, that it cannot be proved to do so.

"It is easy to understand how such ideas of spontaneous generation should have been prevalent in ancient times. Even their repeated occurrence in modern times, and with our modern knowledge, is also capable of explanation. It must be assumed that organisms did once come into being of themselves without parents, being produced from organizable, but yet not organized, matter. It must, moreover, be allowed that this may still happen at any moment, and perhaps does actually happen; its impossibility cannot be proved. To produce actual proof of an original formation of a living being is a matter of the highest interest, and has as powerful an attraction for the biological investigator as the prospect of producing the homunculus in the phial for the alchemist. But the experience of centuries has shown that wherever the homunculus really appeared in the flask it proved itself to be a small imp which had been secretly introduced into it from without; and, speaking seriously, the result was always of this kind. In every single instance exact investigation has shown that the organisms which were supposed to have had no parents proceeded from germs produced from parents of the same species as themselves; it has also been shown how they were formed and whence they came. . . . That there is no generation without parents is a matter of experience; it is in distinct accord with the present state of our knowledge after making allowance for all conceivable possibilities . . ."

What, then, are the organisms which have given rise to such curious notions regarding their mode of origin? We have instanced the mushroom as an example; and to it the toadstools and moulds may be added.

The vegetable kingdom is generally divided into five main divisions, of which the lowest is the Thallophytes (θαλλος "a young shoot," and φυτον "a plant"). The plants of this class show no differentiation into root, stem, and leaf, as is the case in the majority of green plants, but instead are composed of a *thallus*, or body in which no distinction can be made between stem and leaf, and in which a root, such as we are accustomed to in the higher plants, is absent. In this division the Fungi find their place.

It would be impossible in the limits of a single paper to attempt a general description of even the chief members of the various divisions of this enormous group. It will best serve our purpose to confine ourselves to a detailed account of the life of an individual member of one of these. The physiology of the Fungi will, we trust, form the subject of a subsequent paper. Here it will merely receive a passing glance.

We select for our description a Fungus which causes *rust*, or mildew, on grass. Its life-history is extremely complicated, and is intensely interesting. It is a typical *lusus naturæ* of the old authors. The different stages in its interesting life may be traced (now that they are known to us) with comparative ease owing to the large size of its members, so that the readers of this article can thus obtain a first-hand knowledge of some at least of its life processes.

This fungus is the possessor of three names—*Uredo linearis*, *Puccinia graminis*, and *Æcidium berberidis*; the reason of which will be evident to the reader by-and-by. *Puccinia graminis* is the one in common use.

The most conspicuous portion of the fungus, and this holds good for almost all these plants, is that portion set apart for the reproduction of the species. The mushroom is a case in point. The vegetative portion—that part, in other words, which is the carrier-in and elaborator of nourishment—is quite inconspicuous. We will deal with it first. It is the fungus body, the thallus or *mycelium* (μυκης "a mushroom"), and corresponds to the "spawn" of the mushroom. It consists of a number of elongated, many-celled threads or hyphæ (ὑφη "a web"), which branch and interlock to form a more or less compacted weft of tissue. *Puccinia graminis* is a parasite, so we must look for this mycelium in the tissues of a living being, or *host*, as it is termed in technical language.

There are five or more unwilling hosts favoured with this unwelcome guest. We will select one of these—*Triticum vulgare*, the Wheat—for our study of the parasite. It occasionally happens during the early summer that a farmer espies a peculiar colouration on the blades in some parts of his wheat-field. He calls this *mildew*. Closer examination reveals the fact that the blades have a rusty appearance, the *rust* taking the form of small linear patches on the leaves. A microscopic examination of a transverse section of a leaf through one of these patches shows that its tissue is permeated by a number of delicate threads (Fig. I. *b.*, *m.*), which run amongst the cells. At

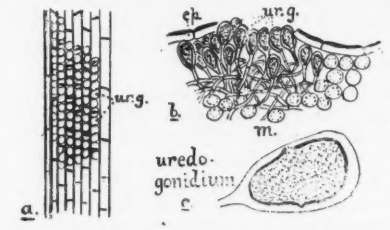

Fig. I.—*a*. Diagrammatic representation of surface of leaf of Wheat (*Triticum vulgare*) slightly magnified. The uredo-gonidia (*ur. g.*) are seen shining through. *b*. Transverse section of leaf of Wheat through a uredo-gonidia patch. *Ur. g.*, uredo-gonidia; *ep.*, epidermis; *m.*, mycelium, the threads (*hyphæ*) of which are woven into a loose tissue. (Much magnified.) *c*. Uredo-gonidium. (Very much magnified.)

one portion the filaments have assumed an upward direction, and there a mass of yellowish cells have made their appearance (Fig. I. *b.*, *ur. g.*). They are so numerous that the epidermis or skin of the leaf has been ruptured. Each one occupies the end of one of the threads. These threads are the hyphæ of the fungus mycelium. They form the thallus, which corresponds to the root, stem, and leaves of a higher plant, such as its host-plant, the Wheat. Many of the wheat-cells are stored with green colouring-matter, or chlorophyll, which enables it to lead an independent existence,* but in the thallus of the Puccinia there is no green colouring-matter; hence it must live on matter which has already been elaborated from inorganic materials by its more favoured host. So far as regards structure, a difference exists between the two, occasioned, of course, by the very different mode of life of the two forms. In the Wheat-plant certain cells are set apart for the performance of certain functions. Each *set* of cells is called a tissue. No such distinction can be drawn between the cells of the fungus mycelium; they are all alike. Each one can take in nourishment for itself from the cells of the leaf in which it has found a home.

Of the yellow cells we have not spoken. They are called gonidia or conidia-gonidia (γεννάω "to produce," and ὀιδα "like"), because they are like spores; true reproductive cells; and conidia (κονις "dust"), from their dust-like appearance. They are not spores, inasmuch as they have

* For explanation, see article on "Breathing Organs of Plants," Knowledge, Nov. 1890.

not been produced as the result of the union of male and female elements. They have simply been cut off, one by one, from the ends of the hyphæ, and have surrounded themselves with a thick wall as a protective covering; but, like spores, they are capable of germinating and producing a mycelium exactly similar to that which gave them origin. The wind catches them and carries them from blade to blade of the wheat. Having alighted thereon, germination commences. A small tube is sent out from each gonidium; it pierces the cuticle or outer coating of the leaf-surface, and insinuates itself between the cells of that body. It probably effects its entrance into the leaf by secreting a ferment which dissolves the hard cuticle. This fermentation is exhibited by many fungi. It enables many of them to penetrate into the hard wood of trees. When the tube has reached the interior of the leaf, it germinates, and the product of germination is a mycelium like that of its parent. On this gonidia may again arise. Two or three generations of gonidia may be produced in this way in a single summer.

The fungus at this stage was formerly called *Uredo linearis*, and its gonidia are still known as *Uredo-gonidia*.

The blade of wheat is a transitory structure, it does not live all winter, but the mycelium of the fungus could not survive that period without a means of sustenance. In point of fact, its existence as such terminates with that of the life of its host; but it makes provision for the continuance of the species, to the cost of the poor farmer on whose lands it has settled.

Towards the end of July the brownish pustules we have previously referred to assume a black appearance, and become more prominent. Now, microscopic examination reveals a curious fact. Where previously uredo-gonidia existed, two-celled bodies with very thick walls have appeared (Fig. II. *a.*, *t.g.*). On some isolated stalks a uredo-

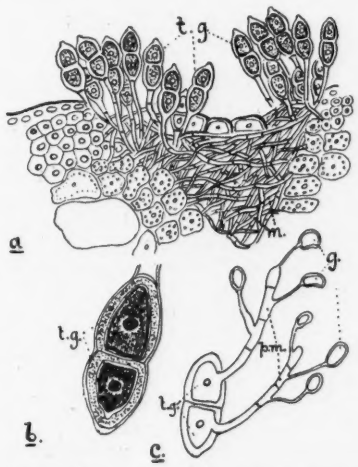

FIG. II.—*a.* Transverse section of Wheat-blade through collection of Teleuto-gonidia (*ad. nat.*); *t.g.*, teleuto-gonidia; *m.*, mycelium. (Much magnified.) *b.* Teleuto-gonidium (*ad. nat.*) (Very much magnified.) *c.* Diagrammatic representation of germination of teleuto-gonidium (*t. g.*); *p. m.*, promycelium; *g.*, promycelial gonidia.

gonidium may still be visible. These are also gonidia, but by reason of their structure are better fitted to withstand the rigors of winter than those produced in the earlier part of the season. They are the last gonidia to appear on the mycelium, so they have received the name of teleuto-gonidia (τελευτος "last").

All this time the fungus has been preying upon its host, and preventing the elaboration of sap for the building up of its seeds. Its attacks are thus dreaded by the farmer. This stage of the fungus, which was not known until recently to have any connection with the preceding, received the name of *Puccinia graminis*.

The mildew, or blight, was known as far back as the time of Shakespeare. In *King Lear*, Act III. sc. iv., we read that "the foul fiend Flibbertigibbet mildews the white wheat." "The fungoid nature of the mildew was not known, however, until the latter half of the last century, for Tull, writing in 1733, attributes it to the attack of small insects, 'brought (some think) by the east wind,' which feed upon the wheat, leaving their excreta as black spots upon the straw, 'as is shown by the microscope.'!"*

The teleuto-gonidia remain on the decayed leaves all winter, but in the spring they throw off their dormant state, become active, and germinate. Each division sends out a small tubular body (Fig. II. *c.*, *p.m.*) which divides up into three or four cells that still retain organic connection with one another. From each cell is thrown out a short process, whose tip develops into a minute bulbous swelling (Fig. II. *c.*, *g.*). The two tubes produced from the teleuto-gonidia are called promycelia, and the swellings just mentioned the promycelial gonidia. These last are produced in immense numbers, and are carried about by the wind.

It was noticed many years ago that in the vicinity of Barberry bushes (*Berberis vulgaris*) the wheat in a wheat-field presented the *rusted* appearance which we now know to be due to the presence of teleuto-gonidia, while in neighbouring parts the wheat retained its normal appearance. Many accounts were given of the connection between the Barberry and mildew on wheat, but no one knew why this connection should exist. So convinced were farmers of the harmful nature of the Barberry that almost all these bushes have been uprooted from our hedges; and in 1760 an Act was passed in Massachusetts enforcing the destruction of these apparently harmless shrubs.

The promycelial gonidia, one would naturally imagine, would germinate on the wheat-leaves, but experience tells us that this does not take place. They will only develop when they fall on the Barberry, or its ally the *Mahonia ilicifolia*. In the leaves of the former plant, and in addition on the berries of the latter, they develop a mycelium in no way differing from that seen in the leaves of the wheat. The hyphæ or threads of the thallus insinuate themselves between the cells, and absorb nourishment from them. Its presence on the Barberry would probably have passed unnoticed were it not that during the early summer reproductive cells arise from the mycelium. Then it is seen that the under surface of the leaves are infected here and there with circular yellow blotches. The aid of a pocket-magnifier shows each blotch to be composed of a zone of small holes, from which a yellow powder issues. On the upper side of the leaves minute apertures are present, arranged likewise in a circular manner. If we examine a section of the leaf under the microscope, the yellow patches will show themselves to be composed of cup-shaped bodies. Every cup is filled with bright yellow cells (Fig. III., *æ. sp.*) arranged in linear series, and each series arises from the down-turned end of a hypha. A distinct wall surrounds each cup or *æcidium* (Fig. III., *æ.*),

* Plowright. *British Uredineæ and Ustilagineæ*, p. 47.

and, like the spores, it is connected with a mycelium in the centre of the leaf. The arrangement of the æcidia in groups has caused them to be called cluster-cups. The mycelium is connected towards the upper surface of the leaf with oval-shaped bodies, spermagonia (Fig. III. *s.*), whose openings to the exterior are the small apertures before-mentioned. They contain rod-shaped cells, the so-called spermatia (Fig. III., *sp.*).

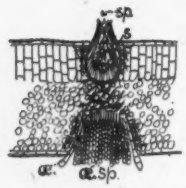

FIG. III.—Transverse section of leaf of Barberry (*Berberis vulgaris*), showing æcidium (*æ.*), with æcidiospores (*æ. sp.*), and spermagonium(*s.*), with spermatia (*sp.*).

As in the previous stages, this phase of the life-history of the fungus was not known to have connection with any of the others, and had a name of its own—*Æcidium berberidis*.

It now remains to determine the origin and use of these respective parts. The Aphis, or plant-louse, has the peculiar power, possessed, however, by other lowly animals, of producing numerous successive broods of young from unfertilized eggs. This is called parthenogenesis (παρθενος "a virgin"; and γεννάω "to reproduce"). On the prothallus* of the Fern antheridia and archegonia may be developed, but from the ovum, or egg, in one of the latter a young fern-plant may arise without union with a spermatozoid; nay, further, without even the production of an ovum a fern-plant may be developed on it. As it is usual for fertilization to take place, these abnormal developments are termed *apogamous* (ἀπο "from"; and γαμέω "to marry"). In a group of Fungi allied to that (*Æcidiomycetes*) to which Puccinia belongs, viz. in the Lichens, spermatia are developed. One fertilizes a female organ, *and the result of their union is a cup-shaped body very much like the æcidium of Æcidium berberidis*. No female organ has been found on this fungus, so it is argued that apogamy here takes place. As the yellow cells germinate and produce a mycelium, and as they have resulted from the union of male and female elements, they are called spores, in contradistinction to the asexually-produced reproductive cells, gonidia. It was believed until lately that the so-called spermatia were male cells, which were now functionless on account of apogamy always happening in *Æcidium berberidis*; but this view of their nature must now be cast aside, for Prof. Plowright has succeeded in causing these to germinate. Their true nature at the present time is unknown.

The inability of the promycelial gonidia to germinate on the blades of Wheat is a matter of wonderment, but the same peculiarity is displayed by the spores (*æcidiospores*) found in the æcidia, or cups of the Berberis. They will germinate only when they reach a blade of Wheat, and not on the Berberis leaf from which their parent mycelium derived its sustenance. Their germination presents features akin to that of the uredo-gonidia and teleuto-gonidia. A tube is sent out, which in this case, however, enters the wheat-blade by way of a stoma. In the interior of the leaf it branches and produces a mycelium, on which uredo-gonidia are produced.

We have thus arrived at the stage of the life-history with which our description commenced. This fungus then passes part of its existence on one plant and part on another, and is parasitic on both. It is thus said to be metoxenous (μετα "change"; and ξενος a "guest"); or, in other words, to change its host. The term heterœcism (ἑτερος "other," and οἰκος "a house") is also used in connection with it, implying that it effects a change of residence during the course of its life. The diagram (Fig. IV.) will

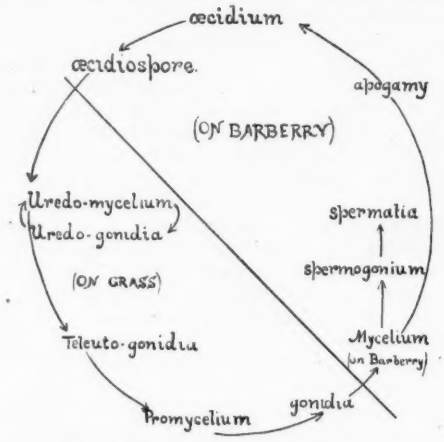

FIG. IV.—Life-cycle diagram of *Puccinia graminis*. As the uredo-mycelium produce uredo-gonidia, which again produce a mycelium like the parent, the life-cycle is at this stage lengthened, while on the other side apogamy causes shortening of the cycle.

serve to make clear the wanderings and vicissitudes of this lowly organism.

It must not be imagined that all Fungi have such a complicated life-history. In many cases it is very simple. Moreover, all are not parasitic; some are saprophytes. They live, like the mushroom, on dead organic matter, which originally was built up by living beings.

* For explanation of terms see article on "A Seed, and what it Contains," KNOWLEDGE, April 1891.

Notices of Books.

Soap Bubbles and the Forces which mould them. By Prof. C. V. Boys, F.R.S. (Society for Promoting Christian Knowledge.)—This very fascinating little book is calculated to start old people as well as young in a course of experimenting for themselves. It is written in very popular language, being the substance of three lectures delivered to a juvenile audience in the Theatre of the London Institution. Prof. Boys is a very accomplished experimenter, with a happy gift of devising simple mechanical contrivances to illustrate his meaning. Those who were not fortunate enough to hear the lectures will find ample woodcuts and other illustrations to make the meaning clear, and at the end of the book Prof. Boys has given a series of practical hints as to bubble-blowing and making some of the simple apparatus which he used. The importance can hardly be overrated of inducing young people to experiment for themselves. It teaches them to observe and to reason for themselves, and is a very important adjunct to the training of the memory which is now too exclusively looked upon as education. Possibly in some cases Prof. Boys has been a little too daring in his attempts at explanation. Thus, in trying to explain that the section of a film between two parallel discs is a catenary when the pressure on the inside is equal to the pressure on the outside, he shows his child audience the sections of a cone by throwing the shadow of a flat candlestick on the wall, and then tells them that he will trace a catenary by rolling upon a straight edge a piece of board, cut into the form of a parabola, letting a piece of chalk, at its focus, trace the catenary on the black-board. But with a

few such small exceptions, the book is exceedingly simple as well as amusing.

The Autobiography of the Earth: A popular account of Geological History. By the Rev. H. N. HUTCHINSON, B.A., F.G.S. (Edward Stanford, London, 1890). There is a scarcity of popular works on Geology which can be relied upon as giving accurate information in simple language. This book has been very conscientiously written. It gives a brief sketch of the principal geological periods, and endeavours to explain briefly the methods by which the conclusions of geologists have been arrived at.

Monograph of the British Cicadæ. Part V. By G. B. BUCKTON, F.R.S. (Macmillan & Co.) This part contains the *Acocephalidæ* and the first two genera of the *Jassidæ*. There is also a useful chapter on the parasites of the Cicadæ, for which no hint of apology was necessary, for in our opinion it is just the discussion of side issues such as these that, next to the figuring of the species, constitutes the chief *raison d'être* of the work. The parasites of the Homoptera have been so little studied in this country that all available information will be welcomed by students of the order. The most interesting subject noticed in this connection is the investigations of Prof. Mik and others into the nature of the black oval objects which so often disfigure the bodies of the *Jassidæ* and *Typhlocybidæ*. These have been proved to yield hymenopterous parasites belonging to the extraordinary ant-like genus *Gonatopus*, but many points in the life-history of the parasites still need elucidation, and it is to be hoped that Mr. Buckton's summary of results may lead others to take up the subject. The coloured plates are not all of equal merit, and the figures of one insect at least, the handsome and brilliant *Platymetopius*, suffer from the unusual defect of being insufficiently coloured.

Nature's Wonder-Workers. By KATE R. LOVELL. (Cassell & Co.) Another book on the inexhaustible theme of insect life and habits. Though the authoress does not profess to write in other than a somewhat popular style, her work is carefully done, and shows an avoidance of those blemishes of style that too often disfigure works of this sort. Into 280 agreeably written and neatly printed pages has been collected a considerable fund of information, for the most part accurate, about some of the familiar insects that more or less directly affect the welfare of mankind. The book is most attractively got up and illustrated with abundance of woodcuts, which are full of life and vigour, and portray many and varied phases of insect life.

By Track and Trail: a Journey through Canada. By EDWARD ROPER, F.R.G.S. (W. H. Allen & Co., 1891.) The author of this chattily written book makes no pretensions to be a naturalist or scientific traveller, but he is an artist with an eye for birds and beasts, as well as for the grand scenery of the north-west. He introduces his readers in a realistic way to a wide range of country extending from the Atlantic to the Pacific, and to many types and conditions of men, taking us through the great wheat-growing country to prairie ranches and the cañons and passes of the Rocky Mountains on to British Columbia. His picture of a Chinook village on Queen Charlotte's Island, named Skidegate, shows the gigantic carved posts which these Indians erect in front of their houses. Speaking of these totem posts Mr. Roper says, "I had always been under the impression that the totem was held sacred by the tribe or family possessing it, and that they would not injure or allow to be injured the object chosen as the symbol of their tribe; but at Skidegate their totem is the shark, yet this family of Indians are famous shark slayers, killing them for profit." The houses of these Indians are most substantial wooden framed structures, sometimes forty feet long and equally deep—the walls and the roof are made of split slabs of cedar. It is surprising what immense beams and posts they use for these houses. When an Indian wishes to build a house he gets up a "bee." Calling the members of his tribe together he distributes presents amongst them, which are called "potlach." They set to work with a will, drawing and fitting together great logs until the house is completed. One of the most striking features of these Indian villages are their totem posts, which are sometimes three or four feet in diameter and forty feet high. They are carved with great grotesque faces and figures and a representation of the animal representing the clan. Mr. Roper likens the posts to a family coat-of-arms, which also records incidents in the family history.

THE COAL-SACK REGION OF THE MILKY WAY.

BY A. C. RANYARD.

NEAR to the foot of the Southern Cross there is a dark patch or hole in the Milky Way which was named by the early English-speaking navigators of the Southern Seas "The Coal-Sack," on account of its blackness as compared with the surrounding region. It is so striking an object in the Southern skies that even the Australian natives seem to have noticed it. It is stated, on the authority of a paper read before the Royal Society of New South Wales,* that this black patch figures in Australian folk-lore as the embodiment of evil in the shape of an emu who is lying in wait for an opossum that has climbed into the branches of a tree represented by the stars of the Southern Cross.

The physical meaning of this dark area in the midst of a closely star-strewn region has long been a subject of speculation. It has generally been described as some 8° or 10° long by 5° or 6° wide, with sharply-defined borders, and as perfectly devoid of stars. Proctor rejected as highly improbable the idea that there could be a tunnel directed towards the earth through a great thickness of stars, and accounted on his stream theory of the Milky Way for the dark patch as an opening where we look into distant space between two branches of the great galactic stream of stars, which he conceived to be a stream of roughly circular section. In an article on the Coal-Sack Region, published in KNOWLEDGE for May 1st, 1886, p. 225, he asks the reader to consider whether it can be "an accident that over this large dark space, covering about 50 square degrees, there is not a single lucid star, while all around its borders lucid stars are strewn in plenty?" The whole surface of the heavens, he remarks, "exceeds the Coal-Sack some eight hundred times in extent; and as there are about 6000 lucid stars, one might expect seven or eight such stars to be found in the Coal-Sack. But this is far from being all. The neighbourhood of the Coal-Sack is much richer in lucid stars than other regions in the heavens; so that it is just where stars should be most richly distributed that this vast black spot makes its appearance." The question whether the absence of naked eye stars from the Coal-Sack Region and their presence in great abundance in the Milky Way Region around can be a mere coincidence can hardly be regarded as doubtful, and every thoughtful person will acknowledge that the observed distribution tends to show an intimate connection between the naked eye stars and the distribution of the smaller stars or nebulous matter, which gives rise to the hazy stream of light we know as the Milky Way.

* See Miss Clerke's *System of the Stars*, p. 356.

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The photograph of α and β *Crucis* and the Coal-Sack Region which we are enabled, by the kindness of Mr. H. C. Russell, Director of the Sydney Observatory, to lay before the readers of KNOWLEDGE, shows that the Coal-Sack is far from being absolutely void of stars; the greater part of it is bridged over by a network or veil of stars which are relatively minute compared with the larger stars that are scattered over the region surrounding the Coal-Sack. This network of minute stars does not cover the whole of the Coal-Sack area. There are small regions, one notably towards the northern end of the Coal-Sack and one near to α *Crucis*, which show a comparatively black and starless background. It is evident that the structure of this portion of the Milky Way cannot be accounted for by the simple cylindrical streams imagined by Mr. Proctor.

The relative blackness of the Coal-Sack Region, as seen by the naked eye, seems to be due to a breach in a network or structure of larger stars, which are evidently distributed in a different manner to the network or structure of small stars which stretches across the Coal-Sack Region. The two systems or groupings of stars need not necessarily lie at greatly different distances, but they seem to be as independent of one another as the distribution of two flocks of birds. There may be some general law connecting the association of two flocks or flights of birds, and yet each may have its own independent arrangement, as for example, one set of larger birds might be attacking or following a flight of smaller birds or insects. We should then expect to find the two flights intermixed as seen on the sky and yet distinctly separate both by their grouping as well as by their size. Thus we find in the photograph stream lines of minute stars as well as stream lines of stars of larger magnitude.

In this region of the Milky Way, stars of similar magnitude seem generally to be associated together in the stream lines. There is a very remarkable series of such diverging stream lines about the bright star α *Crucis* (the lowest of the bright stars on the plate). A circle of small stars may be traced around the large star, within the sharply defined bright ring, which, as was explained in KNOWLEDGE for July of last year, p. 174, is due to the reflection of the light of the bright star from the back of the plate.* Converging towards this circle of minute stars are several curving streams of minute stars, which it seems hardly possible to doubt are associated together. The symmetry of their arrangement with respect to the large star is also very curious. From a probability point of view, it is overpoweringly evident that these diverging streams of small stars, together with the circular stream of small stars from which they spring, form a cluster and are all at about the same distance from us. The fact that α *Crucis* falls apparently at the centre of this curiously symmetrical cluster may be a mere coincidence, and may not indicate any physical connection between the cluster and the large star, but the improbability of such a chance coincidence in position is considerable. The evidence, whatever it is worth, is another link in the chain of evidence tending to prove that the girdle of large stars referred to in my paper in the last number is intimately associated with the Milky Way, and that α *Crucis* and the other stars of the girdle are giant suns compared with the cloud of smaller suns which compose the Milky Way. α *Crucis* is a double star with components 5″ apart which move about one another very slowly, for their position seems hardly to have changed since Sir John Herschel observed them in 1834. They give a spectrum of *Sirian* Type. The stars of the cluster about α *Crucis* seem to belong to the class of smaller stars which stretch across the Coal-Sack; one of the branches of the cluster seems to merge into a curiously curving stream of small stars, which stretches a long way if not right across the Coal-Sack area. There is also, on a glass positive sent me by Mr. Russell, a narrow black channel, bordered by small stars, which stretches right across the Coal-Sack area.

The other photograph reproduced represents the Milky Way, about η Argus, a region which Sir John Herschel thought was composed of stars of larger average magnitudes than other parts of the Milky Way (see *The Cape Observations*, p. 33). He also thought that it differed from other regions of the Milky Way, in that the stars appeared distinctly separated on a perfectly dark background. It is seen here to be composed of a series of clusters separated by a number of darker patches and lanes. It seems clear that the Milky Way is by no means homogeneous; different parts exhibit great differences of star grouping—just as one region of the Milky Way is rich in red stars, while another is rich in stars having bright line spectra, so one region seems to be composed of large stars, and another of relatively small stars, and the method of their grouping in different regions is also strikingly different. This was long ago noticed by Sir John Herschel. He says,† speaking of the region at 17h. 50m.: "The Milky Way is here composed of separate, or slightly or strongly connected clouds of semi-nebulous light, and as the telescope moves, the appearance is that of clouds passing in a *scud*, as the sailors call it . . . . The Milky Way is like sand, not strewed evenly as with a sieve, but as if flung down by handfuls (and both hands at once) leaving dark intervals." The nebula about η *Argus*, though comparatively brilliant in the telescope, seems to be deficient in actinic or photographic light, and consequently to need very long exposures as compared with the great nebula in *Orion*. Mr. Russell says that he can trace on his original negative curving structures, which he says remind him of the great nebula in *Andromeda*. Only three stars are shown in the great dark oval patch in this nebula where Herschel's drawings give four. The dark region, known as the Lemmiscate, also appears quite void of light and of stars, except the one small star observed by Herschel, but the scale of the photograph is too small to show much detail. The scale of both photographs is $1° = 0{\cdot}566$ inch. They were taken with a Dallmeyer six-inch portrait lens attached to the mounting of the telescope, which will be used for taking part in the photographic survey of the heavens.

* During the exposure of the photographic plate, a point on the sensitive film was lit up by the image of the bright star, and shone like a little lamp. The rays from this illuminated region, which fell nearly perpendicularly on the back of the plate emerged, whereas those which fell more obliquely would be in greater and greater proportion reflected back again, and produce their effect on the sensitive film. The inner edge of the bright ring corresponds to the light reflected at the critical angle where the whole of the light is reflected back again to the sensitive film, and none gets out at the back of the plate.

† *Cape Observations*, p. 388.

FLYING ANIMALS.

By R. LYDEKKER, B.A.Cantab.

ONLY in certain members of two great groups of animals do we meet with the faculty of flight, or the power of supporting their bodies in the air for longer or shorter periods by the aid of membranous or other expansions developed therefrom. These two groups are the Insects, which constitute a class of the larger group known as the Arthropodous sub-

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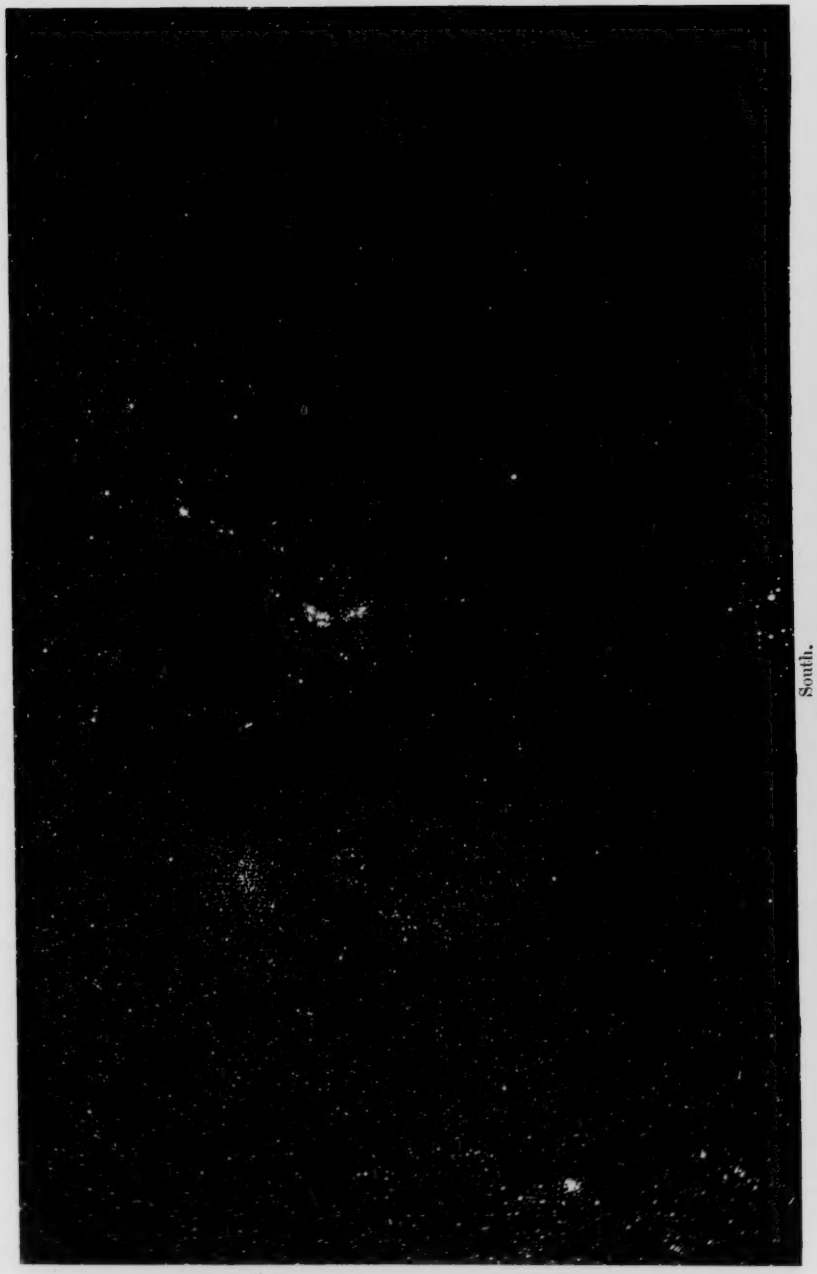

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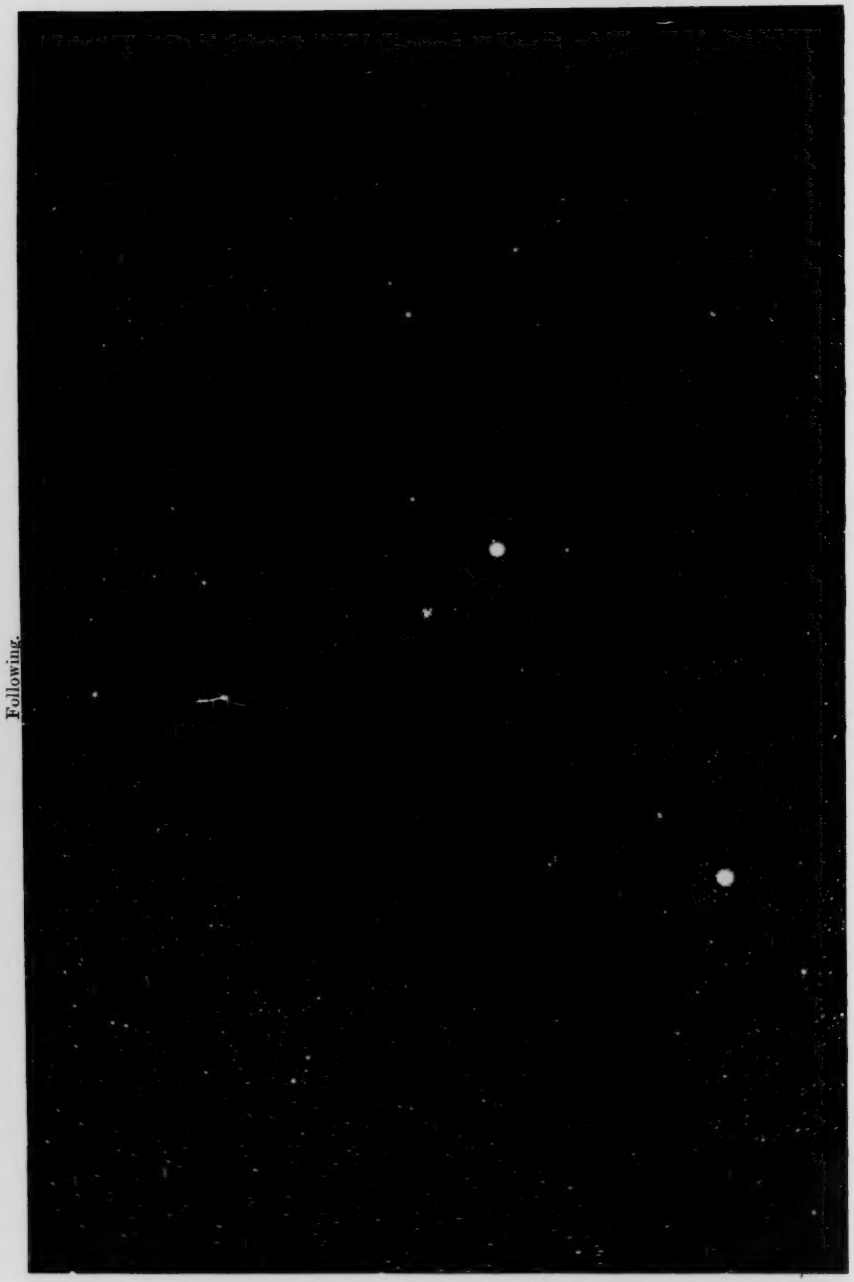

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THE η ARGUS REGION AND NEIGHBOURING CLUSTERS IN THE SOUTHERN MILKY WAY.

From a Photograph taken by Mr. H. C. Russell, Director of the Sydney Observatory. Exposure, 3 hours. 23rd July, 1890.

α AND β CRUCIS AND THE COAL-SACK REGION OF THE MILKY WAY.

From a Photograph by Mr. H. C. Russell. Exposure, 3 hours. 13th August, 1890.

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kingdom, in which are also included spiders and crabs; and the Vertebrates, which form a sub-kingdom to themselves. Whereas, however, by far the great majority of Insects are endued with this faculty, among the Vertebrates it is only in the class of Birds that we meet with a similar preponderance of species which enjoy this kind of locomotion, although all the members of certain orders—the Pterodactyles and Bats—are similarly endowed. Moreover, we have to draw a distinction between true flight, as exemplified by Birds, Bats, and Insects, and what we may call spurious flight, of which we have examples in Flying Phalangers, Flying Squirrels, and Flying Fish. True flight is performed by an alternate upward and downward motion of the wings, or special organs of flight, and can be indefinitely prolonged until the muscular powers of the flyer are exhausted. On the other hand, spurious flight is merely a prolongation of a downward or upward leap by means of parachute-like expansions developed on the sides of the body, or, as in the Flying Fish, by passive extension of wing-like organs, and it can never be extended beyond the limits of the initial velocity of the original leap. This distinction between true and spurious flight is a very important one, since it shows us that the animals endowed with the former power are limited to four groups, namely, Insects, the extinct Pterodactyles or Flying Dragons, Birds, and Bats. Spurious flight, on the other hand, is found in Flying Fish, Flying Lizards, Flying Phalangers, Flying Squirrels, and Flying Lemurs. Among those animals capable of true flight a broad line of distinction separates the Insects from the Vertebrates in regard to the organs set apart for this particular purpose. Thus, whereas in Insects, all of which are provided with six pairs of legs, the wings, or special organs of flight, are frequently four in number, and are in all cases developed from the back of the body, entirely independent of the legs; in Vertebrates, where the number of legs never exceeds four, the two wings are always formed by special modification of the first pair of legs. It is therefore evident that although the wings of Insects, as performing similar functions, are analogous with those of Vertebrates, yet, as being structurally quite different, they are in no sense homologous with the same.

The special modification of the first pair of legs to subserve the purpose of flight in those Vertebrates which possess this power in its true form, may be taken as an indication that such Vertebrates have originally descended from others in which that power was not developed. Although we have no such guide in the case of Insects, yet the circumstance that in all those kinds which undergo a complete metamorphosis no traces of wings are observable in their larvæ, points with equal clearness to the conclusion that these creatures have been likewise derived from crawling ancestors, and that their power of flight is an acquired one. Those Insects which are unable to fly must not, however, be regarded as ancestral forms, since there is clear evidence that their wings have been lost or have became rudimentary. It has been already mentioned that while all flying Vertebrates have only a single pair of wings, many Insects are provided with two pairs of these organs; and from the tendency among Insects for one or other of these pairs of wings either to disappear or to be modified for other purposes, it would appear that a single pair is decidedly the best suited for flight.

We shall now proceed to trace some of the chief modifications in the organs of flight in the different groups of animals, commencing with Insects, in which, as already observed, we never meet with spurious flight.

In all the Beetles, or Coleoptera, which form the first order of Insects, the front pair of wings are modified into the well-known horn-like wing-covers, or *elytra*, beneath which the membranous second pair are neatly folded during such times as the creatures are not engaged in flight. In some kinds, such as the Stag-Beetle and the Water-Beetles, these wing-cases are long, and extend backwards to the hinder extremity of the body; but in others, like the well-known "Devil's Coach-Horse," they are extremely short. The modification of the front wings into wing-covers clearly indicates that Beetles are a highly specialised group; the extreme development of this specialisation occurring in certain species like the Oil Beetle, where the second pair of wings have also become rudimentary, so as to render their owner incapable of flight. In some degree a confirmation of this specialisation is afforded by the circumstance that Beetles are not known in the fossil condition so far down in the geological scale as are some of the more generalised groups of insects.

In the Bees, Wasps, Ants, and other members of the second order Hymenoptera, both pairs of wings are membranous and adapted for flight; the front pair being, however, considerably the larger of the two. In the Caddis Flies and other Neuroptera, both pairs of wings are likewise fully developed and membranous in structure, although differing in the mode of arrangement of their veins. Moreover, the hinder pair are frequently nearly as large as the front ones, a circumstance which seems to indicate that the whole group is a more generalised one. The development of the well-known minute scales on both pairs of wings in the Butterflies and Moths readily distinguishes the Lepidoptera from all other insects, and likewise suggests that they form a much specialised modification of the class. Still greater specialisation as regards their organs of flight is, however, presented by the Flies and Gnats, constituting the order Diptera, in which, while the front pair of wings are large and membranous, or hairy, the second pair are reduced to small, drumstick-like processes termed balancers, or *halteres*, which are of no sort of use in flight, and are typical rudimentary organs. The specialisation of the wing-structure in this group is, therefore, exactly the opposite of what occurs among the Beetles, where, as we have already seen, it is the first pair of wings which takes no part in flight. In the Cicadas and Bugs (Fig. 1), constituting the order Rhynchota, the wings, when present, are four in number, but the first pair may be converted into horny wing-covers, as in the Beetles. Like those of the next order, all the members of this group differ from the insects mentioned above in that they do not undergo a complete metamorphosis before attaining their final perfect state. The last order that we have to notice is the Orthoptera, in which are grouped the Grasshoppers, the Cockroaches, the Earwigs, the Dragon-flies, &c. Except in a few parasitic and some other

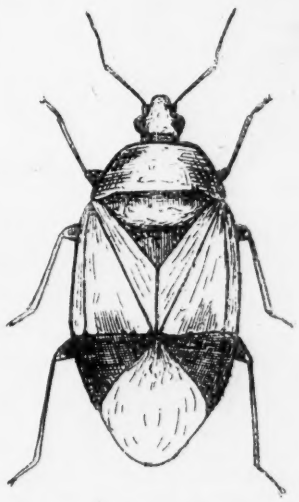


FIG. 1.—ENLARGED VIEW OF A FLYING BUG, WITH THE WINGS CLOSED.

forms, all these insects are furnished with two pairs of wings, which differ, however, greatly in structure. Thus, while in the Dragon-flies, which in this respect may be regarded as the more generalised representatives of the order, both pairs of wings are large and membranous; in the Grasshoppers, Cockroaches, and Earwigs the front pair are leathery, and serve as wing-covers to the hinder pair, which are folded beneath them in a beautiful, fan-like manner. Whereas, however, in the Grasshoppers the first pair of wings still take some small share in flight, in the Earwigs they are extremely small, and serve solely as covers. The Earwigs, therefore, which many people believe to be incapable of flight, represent the extreme of wing-specialisation in this group of Insects.

This closes the list of flying creatures found among the Invertebrates, and we pass, therefore, to the Vertebrates, where we find our first examples of flight among the class of Fishes. In this group, however, in spite of assertions to the contrary, there is no instance of true flight; such fishes as are able to fly at all merely doing so after the spurious manner. The longest flights are made by the

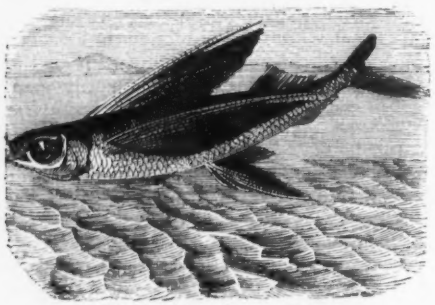


Fig 2.—The Flying Fish

well-known Flying Fishes (Fig. 2) of most of the warmer seas, in which the first pair of fins are greatly elongated for this purpose. These fishes rise from the water with an upward impulse made by the sides of the body and tail, and they may remain above the surface for a distance of 200 yards. They do not usually reach a height of more than a few feet above the water, although they occasionally spring so high as to alight on the decks of ships. There are few more beautiful sights than to watch from the bows of a large ocean steamer a shoal of Flying Fish as they rise one after another, with their quick meteor-like flight, and then as suddenly disappear beneath the dark waters.

Flying Fish, it may be observed, are first cousins of the common Herring. The only other Fish endued with the power of flight are the Flying Gurnards, which belong to a totally different group, and of which there are three kinds inhabiting the Mediterranean and most tropical seas. All of them are larger and heavier than the true Flying Fishes, although they fly in the same manner.

It has been stated that a Frog from the Malay region uses the large webs on its feet as a kind of parachute in its descent from the trees on which it dwells to the water, but later researches do not lend countenance to this idea; and our next examples of flight must accordingly be drawn from the class of true Reptiles. Among living Reptiles there is no instance of true flight, although two groups are endowed with the power of spurious flight. The first example of this is the Flying Gecko, a small lizard, belonging to that peculiar group so well known in tropical climates from their habit of running up and down the walls of dwelling-houses. The Flying Gecko is an inhabitant of Borneo, Java, &c., and attains a length of about 7 inches. Its sides, limbs, tail, and head are furnished with loose expansions of skin which, becoming inflated with air, act as a parachute in the long, flying leaps which the creature is able to take from tree to tree. The true Flying Lizards, which range from India to the Philippines, have their parachutes constructed after a totally different fashion. In these creatures the last five or six ribs are greatly elongated to support an expansion of the skin of the flanks, which forms a fan-like wing on either side. The late Prof. Moseley described these lizards in the Philippines as flying so rapidly from branch to branch that the extension of their parachutes could scarcely be observed; and also states that some kept on board ship were in the habit of flitting from one leg of the table to another.

Since the extinct Flying Dragons or Pterodactyles of the Mesozoic epoch, which are the only reptiles capable of active flight, have been described at length in a previous article, our allusion to them will here be brief. These extraordinary creatures, as shown in Fig. 3, were furnished with thin membranous wings, which were supported in front by the arm and fore-arm near the body, and at their extremities by the greatly extended joints of a finger corresponding either to the ring or little finger of the human hand. The membranous expansion was continued down the sides of the body to embrace the legs and the upper part of the tail; while in at least some of those species in which the tail was long, its extremity was furnished with a racket-shaped expansion of membrane (as in Fig. 2), probably used as a kind of rudder during flight. Some of these creatures were of enormous dimensions, having an expanse of wing estimated at upwards of 25 feet. That they were endowed with the power of true flight is perfectly evident from their general structure; as is especially shown by the strong ridge developed on the breast-bone for the attachment of the muscles necessary for the down-stroke of the wings. Their mode of flight was probably very similar to that of Bats, which they appear to have resembled in their wing-membranes, although the support of these membranes, as we shall subsequently see, was arranged on a totally different plan in the two groups. It is, perhaps, superfluous to add that any resemblances existing between Pterodactyles and Birds are solely due to their adaptation to a similar mode of life, and that there is not the remotest genetic connection between them.

Fig. 3.—Restoration of a Long-Tailed Pterodactyle. ¼th Natural Size. (*After Marsh.*)

We come now to the Birds, in which true flight has attained the fullest development, and the whole organization is profoundly modified to suit the exigencies of a more or less completely aërial mode of life. It is true, indeed, that certain birds, such as the Ostrich, Cassowary, and Penguins, are totally incapable of flight; but this

incapacity is certainly an acquired one in the last-named bird, and there is a considerable probability that it was likewise so in the two former.

The great peculiarity whereby Birds differ from all other animals is in the presence of their external covering of feathers. A feather, as we all know, is one of the most beautiful objects in nature; and its structure, which we may, perhaps, explain in a later article, is an admirable instance of adaptation for a particular purpose. The uses of feathers are two-fold. In the first place, the small ones with which the body is clothed form the most perfect covering that can be imagined to ensure the maintenance of the high bodily temperature so essential to the active existence of a bird. Then, again, the larger and stronger feathers of the wings are the most efficient instruments for obtaining the utmost advantage from the resistance of the air to their strokes during flight. The peculiar nature of the wings of Birds may be summarised by the statement that whereas all other animals fly by means of expansions of the skin itself, these alone fly by means of separate outgrowths or processes developed from the skin.

(To be continued.)

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

To the Editor of KNOWLEDGE.

Mackay, Queensland, March 21st, 1891.

Sir,—Is there any *English* work treating of Astronomical Photography?

In G. L. Chambers' "Handbook of Astronomy," Vol. II., 1890, p. 416, a footnote mentions a German work, Konkoly's, published by Halle, 1887, as the only work on the subject.

People who live in the centres of population can, no doubt, get *vivâ voce* instruction; those in distant parts of the world have to depend upon books, and there seem to be none on this subject.

I am one of those who deeply regretted the cessation of Answers to Correspondents in 1885, and of Gossip in 1888, which alone were worth, to me, twice the money paid for KNOWLEDGE. I liked the genially caustic pen of the late lamented R. A. Proctor, and his way of making personal friends of his readers. Your obedient servant,

J. GWEN DAVIDSON.

[We have not at present, as far as I am aware, any book in English on Astronomical Photography. A very charmingly illustrated little French book on the subject was issued in 1887 by Admiral Mouchez,* the Director of the Paris Observatory, but it is devoted to giving an account of what has been done, rather than to answering the questions which I imagine Mr. Davidson would like to ask. The photographic difficulties which the astronomical photographer will encounter are dealt with in a legion of text-books. The astronomical difficulties involved in the accurate mounting of his equatorial, and the accurate driving of his clock, cannot, I think, be dealt with by any copy-book rules. To succeed, the astronomical photographer must be naturally an experimenter, his difficulties must be thought out and conquered as they arise. He will soon find the exposures necessary to obtain the best effects. For the Moon, Jupiter and Saturn, in the principal focus, only a fraction of a second is necessary, depending on the aperture and focal length of the telescope, as well as on the altitude of the object above the horizon and the clearness of the night. Hence, these objects, as well as the Sun, can be photographed without a driving clock. Dr. von Konkoly's† book on Astronomical Photography contains a good many woodcuts of instruments and apparatus, but they are not such diagrams that a reader who did not know what was represented could construct an instrument from. This book is also four years old, and Astronomical Photography has made great progress since that time. I would advise any intending astronomical photographer to thoroughly read some book on geometrical optics, and then to think out his difficulties for himself.—A. C. RANYARD.]

To the Editor of KNOWLEDGE.

DEAR SIR,—Whilst grateful to you and your reviewer for pointing out the mistake in the last edition of my *Celestial Motions* (I might, perhaps, demur to some of the remarks, but do not propose to do so at present), allow me to call your attention to an error in the same number of KNOWLEDGE itself (p. 91), which may puzzle many readers. The passage runs: "He [Michell in *Phil. Trans.* for 1767] concludes that there must be some physical connection between the numerous double and triple stars which had already been discovered by Sir William Herschel . . . ." Michell makes no mention in his paper of Herschel's discoveries, nor could he for a similar reason to that which prevented a distinguished personage from seeing a Spanish fleet. It was not in sight; and Herschel's discoveries had not commenced in 1767, the year after he was appointed organist in Bath.

Yours faithfully,

Blackheath, May 6th, 1891. W. T. LYNN.

[Michell's reference to the double and triple stars discovered by Herschel, is in a paper published in the *Phil. Trans.* for 1784. The fact to which I wish to call attention is that Michell's remarkable papers were written before Herschel's discovery that several close pairs of stars were moving round one another. The boldness of Michell's conclusion that there must be a physical connection between close double stars is rendered more remarkable by the fact that, at the date of his first paper, 1767, less than a hundred of such pairs of stars were known. Michell's words in his first paper are well worthy of being quoted at length. He says, at p. 247 of the *Phil. Trans.* for 1767, after speaking of certain stars which appear double to the naked eye—

"If, besides these examples that are obvious to the naked eye, we extend the same argument to the smaller stars . . . . which appear double, treble, &c., when seen through telescopes, we shall find it still infinitely more conclusive, both in the particular instances and in the general analogy arising from the frequency of them. We may from hence, therefore, with the highest probability, conclude (the odds against the contrary opinion being

* "La Photographie Astronomique a l'Observatoire de Paris." Par M. le Contre-Amiral E. Mouchez. Paris, Gauthier-Villars, 55, Quai des Grands-Augustin, 1887.

† "Praktische Anleitung zur Himmelsphotographie nebst einer kurgefassten Anleitung zur modernen photographischen Operation und der spectral photographie im cabinet von Nicolaus von Konkoly. Halle, 1887.

many millions to one) that the stars are really collected together in clusters in some places, where they form a kind of system, whilst in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation, or to some other law or appointment of the Creator; and the natural conclusion from hence is, that it is highly probable in particular, and next to a certainty in general, that such double stars as appear to consist of two or more stars placed very near together do really consist of stars placed near together, and under the influence of some general law."

Herschel subsequently greatly enlarged the list of telescopic double stars which he sought for and measured in the hope of finding some in which the distance and position angle might vary in the course of the year, indicating a parallax as the earth moves round its orbit. But instead of finding a yearly oscillation in the distance and position angle as he expected, he found to his surprise, in many cases, a regular progressive change which indicated that one of the stars was slowly describing a regular orbit round the other. This discovery was announced in Herschel's paper in the *Phil. Trans.* for 1803 and 1804. In speaking of this discovery Herschel said that he "went out like Saul to seek his father's asses, and found a kingdom," the dominion of gravitation extending to the stars. A dominion which, it should be noted, the Rev. John Michell had six-and-thirty years before prophesied would be found to exist, and which in his paper of 1784 he had still more confidently asserted must exist.—A. C. Ranyard.]

STATIONARY RADIATION OF METEORS.

To the Editor of Knowledge.

Sir,—I note some remarks in your current number, bearing on the Stationary Radiation of Meteors. The difficulty of reconciling this feature with theory is well known, and the question has been debated whether the fixed radiants cannot be explained by successive showers accidentally placed in nearly the same apparent points of the firmament. After investigating the observational part of the subject as fully and carefully as circumstances allowed, I found that, allowing for trivial errors in determining positions, the showers occurred from identical centres, and certainly could not be ascribed to chance grouping.

A large proportion of the observed radiant points are of this fixed and long-enduring character. As a rule their individual meteors move with great velocity when the radiants are near the earth's apex, but, with increasing distance from it, their speed sensibly moderates, and they finally become slow, and very slow. Thus my stationary radiant at 47°+44° yields *very swift* streak-leaving meteors in July and August, while in November and December they are *very slow*. But this peculiarity is not exemplified in all cases, for there is a shower at 61°+49° which discharges very swift meteors at the end of November and early in January, whereas in September I have recorded them as *swift*. This is, however, an exception, and the rule appears to be that the showers meeting the earth give swift meteors, while those overtaking it give slow ones. It is clear from the varied phenomena observed both in the major and minor systems, that one and the same explanation will not suffice to explain them all, for every possible diversity of meteor shower may exist and display radiation in the firmament. We may find sporadic meteors of great velocity, and coming from distant space. There may be hyperbolic, parabolic, and elliptical streams, also meteors comparatively isolated, and forming the remnants of past groups dismembered by planetary perturbations, for the vicissitudes which these small bodies encounter must be very considerable. Some of the elliptical streams are very wide, and of this the August Perseids, with their shifting radiant, afford a prominent instance. The apparently stationary shower which I have seen at various times, coinciding in position with that of the Perseids on August 10th, is, of course, entirely different in character to the cometary shower, and needs a different explanation. The latter presents a vastly richer display as well as a moving radiant, and these features readily distinguish it from contemporary showers.

We require a vast amount of additional observation in this field. If an energetic observer, living in a finer climate than England, took up the subject, and watched the sky assiduously for several years, he would undoubtedly obtain sufficient data to clear up some of the features which now present such difficulties. I do not think the results of past observation would be controverted, but that the new evidence might enable satisfactory theories to be formed.

As to the accuracy with which radiant points may be determined, I believe that observers of long experience are likely to be the best judges of this. The probable error is different in different cases, for scarcely any two observers exhibit the same degree of skill. Training would never make some individuals accurate in this difficult branch of work. It takes fully two years of habitual observation before anyone can acquire desirable proficiency in recording meteors, and confidence in assigning their radiants. Speaking for myself, I believe my positions are within 2°, and frequently within 1°, of the real centres. I should regard 2° as a *large* error in ascribing the radiant of a well-defined meteor shower. The Andromedes of November 27th, with their widely-diffused radiant, form a very exceptional shower, from which it would be unsafe to judge of the character of others. Many of the minor systems exhibit contracted and sharply-defined radiants which may be accurately determined by the careful observer.

Yours faithfully,

Bristol, May 20th, 1891. W. F. Denning.

METEOR-RADIANTS.

To the Editor of Knowledge.

Sir,—I am afraid that I cannot throw any light on the limits of error as regards the determination of meteor-radiants. It would be interesting if three or four practised observers would make their observations on the same night, at the same place, and then compare their results. But whatever the limits of error may be, I do not see that errors would, on the whole, produce a greater clustering of meteor-radiants than if the positions were accurately known. I should add that Mr. Denning's catalogue contains only positions determined by observations on a single night. But he has a column entitled "Other nights of observation," *i.e.*, other nights on which meteors were observed as coming from the radiant thus determined, which I used in my previous letter. The number of meteors used in determining the radiant is in each case mentioned in his catalogue.

I did not intend to maintain that these stationary or long-enduring radiants are in all cases active throughout the year. The evidence, at present, at least, does not go that far. But the reasons for not observing meteors from particular radiants at certain seasons of the year are pretty evident. We cannot expect to trace such meteors when

the radiant point is below the horizon or near the horizon at the usual hours of observation; and as Mr. Denning tells us that he made nearly all his observations looking East, meteors from radiants in the West would naturally be passed over. Further, I do not think that where the radiant is of this long-enduring or stationary character, the shower is of uniform intensity throughout. On the contrary, so far as I have traced, the maximum of each long enduring shower occurs nearly at the same date in successive years. The showers appear, moreover, to be more intense in some years than in others.

I wish to state in conclusion, that I never disputed in any way the accuracy of Mr. Denning's observations. I only differ from him as to the classification and arrangement of some of them.

Truly yours,

16, Earlsfort Terrace, Dublin. W. H. S. MONCK.

THE FACE OF THE SKY FOR JUNE.

BY HERBERT SADLER, F.R.A.S.

THE increase in solar activity still continues. During June there is no real night in the British Islands. There will be an annular eclipse of the Sun on the afternoon of the 6th, which will be visible as a partial eclipse at Greenwich. At that station the eclipse begins at 5h. 2.2m. P.M., the first contact taking place at an angle of 88° from the vertex towards the west, reckoning for direct image; the middle of the eclipse being at 5h. 46·7m. P.M.; and the last contact taking place at 6h. 23·6m. P.M., at an angle of 5° from the vertex towards the west; the magnitude of the eclipse being 0·238. With the exception of an exceedingly small eclipse on the morning of March 26th, 1895, the next solar eclipse visible at Greenwich will not take place till June 8th, 1899.

The following are conveniently observable times of minima of some Algol type variables (*cf.* "Face of the Sky" for April and May): U Cephei.—June 2nd, 10h. 29m. P.M.; June 7th, 10h. 9m. P.M.; June 12th, 9h. 49m. P.M.; June 17th, 9h. 28m. P.M.; June 22nd, 9h. 8m. P.M. S Cancri.—June 14th, 10h. 22m. P.M. δ Libræ.—June 14th, 0h. 26m. A.M.; June 18th, midnight; June 27th, 11h. 35m. P.M. U Coronæ.—June 20th, 11h. 49m. P.M.; June 27th, 9h. 31m. P.M. U Ophiuchi (17h. 10m. 57s. + 1° 20′).—Max., 6·0 mag.; min., 6·7 mag.; period, 0d. 20h. 7m. 41·60s.—June 4th, 10h. 17m. P.M.; June 9th, 11h. 3m. P.M.; June 14th, 11h. 48m. P.M.; June 15th, 7h. 56m. P.M.; June 20th, 8h. 41m. P.M.; June 25th, 9h. 28m. P.M. Y Cygni (20h. 47m. 47s. + 34° 15′).—Max., 7·1 mag.; min., 7·9 mag.; period, 1d. 11h. 56m. 48s.—June 2nd, 10h. 6m. P.M.; June 5th, 10h. 1m. P.M.; June 8th, 9h. 56m. P.M.; June 11th, 9h. 51m. P.M.; June 14th, 9h. 45m. P.M.; June 17th, 9h. 40m. P.M.; June 20th, 9h. 34m. P.M.; June 23rd, 9h. 28m. P.M.; June 26th, 9h. 23m. P.M.; June 29th, 9h. 18m. P.M. Variable, of short period, not of Algol type. η Aquilæ (19h. 46m. 52s. + 0° 43′). Max., 3·5 mag.; min., 3·7 mag.; period, 7d. 4h. 14m. 0s. June 27th, 9h. P.M. Maximum of R Hydræ (*cf.* "Face of the Sky" for February, 1889) on June 1st. The lines of hydrogen in the spectrum of this star appear bright near maximum.

Mercury is a morning star throughout June, but owing to his proximity to the Sun and the strong twilight prevailing he is not very favourably situated for observation. He rises on the 1st at 3h. 13m. A.M., or 38m. before the Sun, with a northern declination of 13° 26′ and an apparent diameter of $8\frac{3}{4}''$, just three-tenths of the disc being illuminated. On the 16th he rises at 2h. 48m. A.M., or 56m. before sunrise, with a northern declination of 18° 53′, and an apparent diameter of $6\frac{1}{2}''$, six-tenths of the disc being then illuminated. On the 30th he rises at 3h. 9m. A.M., or 38m. before the Sun, with a northern declination of 23° 54′, and an apparent diameter of $5\frac{1}{4}''$, $\frac{95}{100}$ of the disc being then illuminated. He is at his greatest western elongation ($23\frac{3}{4}°$) on the evening of the 5th. During the month he passes from Aries throughout the whole length of Taurus into Gemini, but without approaching any conspicuous star very closely. Venus is also a morning star this month, but her observation is rendered difficult by the same conditions which militate against the visibility of Mercury. She rises on the 1st at 2h. 45m. A.M., or 1h. 6m. before sunrise, with a northern declination of 13° 47′ and an apparent diameter of $11\frac{1}{2}''$, $\frac{87}{100}$ of the disc being illuminated. On the 30th Venus rises at 2h. 24m. A.M., or 1h. 23m. before the Sun, with a northern declination of 22° 1′ and an apparent diameter of $10\frac{1}{2}''$, $\frac{93}{100}$ of the disc being then illuminated, and the brightness of the planet being only one quarter of what it was on January 8th. During the month she passes from Aries into Taurus. Mars is invisible.

The minor planet Vesta (*cf.* "Face of the Sky" for January, 1890) comes into opposition on the 23rd, and but for her great southern declination would be excellently placed for observation, as, with one exception, this is the closest approach to the earth that she has made during the last thirty years. Her distance from us at opposition is about 106,655,000 miles, and she is visible to the naked eye during the whole of June, though the proximity of the nearly full Moon at the actual date of opposition will interfere with naked eye observation. At the present opposition she attains to the 6·0 magnitude, and it is to be hoped that search will be made with powerful telescopes in the South of Europe and the United States, both by means of photography and by eye observations, for a possible satellite. On the day of opposition she souths at midnight with a southern declination of $20\frac{3}{4}°$, her apparent diameter being about $1\frac{1}{2}''$. A map of the path during the month will be found in the *English Mechanic* for May 8th. As Jupiter does not rise till 11h. 9m. P.M. on the last day of the month, and as none of the satellite phenomena are visible at Greenwich till after midnight, we defer an ephemesis of him till July. Saturn is an evening star, but is nearing the west so rapidly that he should be looked for as soon as possible after sunset. He sets on the 1st at 1h. 9m. A.M., with a northern declination of 9° 27′ and an apparent equatorial diameter of $17\frac{1}{2}''$ (the major axis of the ring-system being $40\frac{1}{4}''$ in diameter, and the minor $3\frac{3}{4}''$). On the 30th he sets at 11h. 13m. P.M. with a northerly declination of $8\frac{3}{4}°$, and an apparent equatorial diameter of 16·6″ (the major axis of the ring-system being $38\frac{1}{4}''$ in diameter and the minor 3″). On the 4th Iapetus is near his greatest eastern elongation, where he is faintest. On the evening of the 6th Titan is eclipsed by the shadow of Saturn, the middle of the eclipse taking place about 7h. 30m. P.M., and the satellite is again eclipsed on the evening of the 22nd, the middle of the eclipse being at about 6h. 45m. P.M. Iapetus is about 36″ north of Saturn on the evening of the 23rd. Saturn is in quadrature with the Sun on the 1st, and describes a short direct path in Leo during June, but does not approach any conspicuous star. Uranus rises on the 1st at 3h. 52m. P.M., with a southern declination of 10° 11″ and an apparent diameter of 3·6″. On the 30th he rises at 1h. 55m. P.M., with a southern declination of 10° 1′, and an apparent diameter of 3·6″. He describes a very short retrograde path to the N.N.E. of 86 Virginis during the month. Neptune is invisible.

XUM

There are no very well marked showers of shooting stars in June.

The Moon is new at 4h. 26m. P.M. on the 6th; enters her first quarter at 0h. 34m. P.M. on the 14th; is full at 5h. 12m. A.M. on the 22nd; and enters her last quarter at 11h. 16m. P.M. on the 28th. She is in apogee at midnight on the 13th (distance from the earth, 251,225 miles); and in perigee at 5h. A.M. on the 26th (distance from the earth 228,350 miles). The greatest western libration takes place at 3h. 0m. P.M. on the 7th, and the greatest eastern at 3h. 17m. A.M. on the 20th.

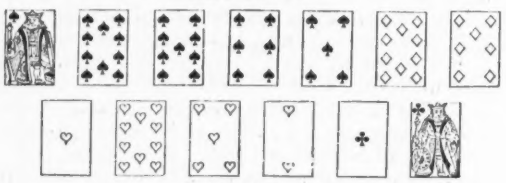
Whist Column.

By W. MONTAGU GATTIE, B.A.OXON.

DECLINING TO DRAW THE LOSING TRUMP.

THE situation in which it is bad play to draw the losing trump is usually defined in the text-books as that in which one adversary has a long suit established, while his partner holds a card of that suit and also the losing trump. Another and somewhat more complicated case arises when the holder of the losing trump, although void of the established suit, can lead another suit in which his partner has a card of re-entry. This is illustrated by the following hand—

HAND NO. 21.



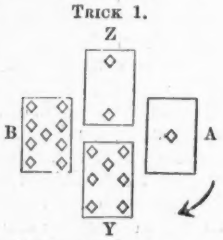
Y's Hand.

Score—Love all.

Z turns up the seven of hearts.

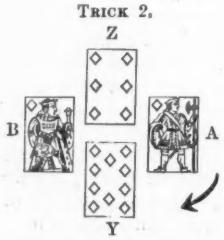
NOTE.—A and B are partners against Y and Z. A has the first lead; Z is the dealer. The card of the leader to each trick is indicated by an arrow.

TRICK 1.



Tricks—AB, 1; YZ, 0.

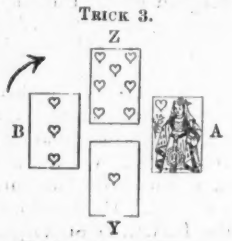
TRICK 2.



Tricks—AB, 2; YZ, 0.

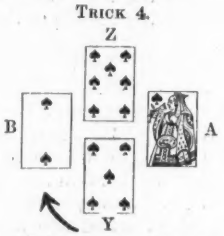
NOTE.—The only diamond Z can have is the eight; A has the rest, unless B has begun a call for trumps.

TRICK 3.



Tricks—AB, 2; YZ, 1.

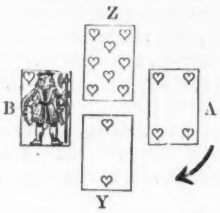
TRICK 4.



Tricks—AB, 3; YZ, 1.

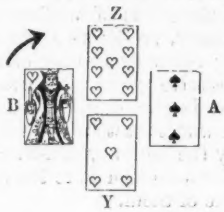
NOTE.—*Trick* 4.—With trumps declared against him, and an adverse suit already established, Y prefers not to open with his fourth-best spade. The fall of the cards shows that B has the knave of spades.

TRICK 5.



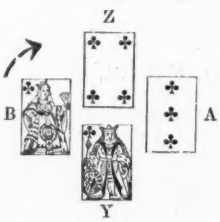
Tricks—AB, 4; YZ, 1.

TRICK 6.



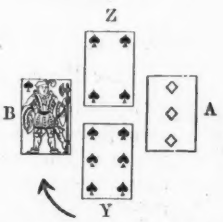
Tricks—AB, 5; YZ, 1.

TRICK 7.



Tricks—AB, 5; YZ, 2.

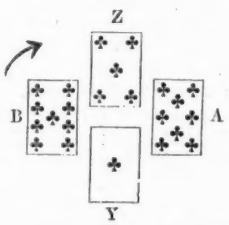
TRICK 8.



Tricks—AB, 6; YZ, 2.

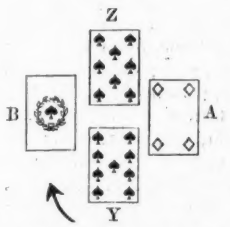
NOTE.—*Trick* 8.—Y retains his trump to ruff the diamonds, in case A should have the ace of spades. After this trick it is clear that A can only bring in the diamonds by winning a trick in clubs.

TRICK 9.



Tricks—AB, 6; YZ, 3.

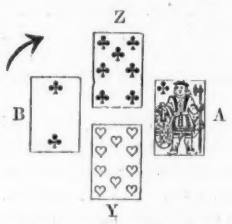
TRICK 10.



Tricks—AB, 7; YZ, 3.

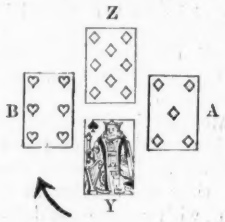
NOTE.—*Trick* 10.—If Y now draws the trump, and A should afterwards be found with the best club, he will make that and the queen of diamonds, and AB will win the game. Z's only possible spade is the eight; and, as he can only have one diamond, he must have four clubs, of which one must be either knave or ten.

TRICK 11.



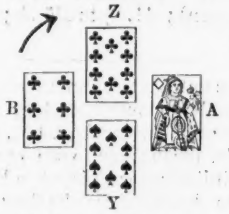
Tricks—AB, 7; YZ, 4.

TRICK 12.



Tricks—AB, 8; YZ, 4.

TRICK 13.



Tricks—AB, 8; YZ, 5.

AB SCORE TWO BY CARDS AND TWO BY HONOURS, AND YZ SAVE THE GAME.

A's Hand.	B's Hand.
H.—Qn, 4.	H.—Kg, Kn, 6, 3.
S.—Qn, 3.	S.—Ace, Kn, 2.
D.—Ace, Qn, Kn, 5, 4, 3.	D.—Kg, 9.
C.—Kn, 8, 3.	C.—Qn, 9, 6, 2.
Y's Hand.	**Z's Hand.**
H.—Ace, 10, 5, 2.	H.—9, 8, 7.
S.—Kg, 10, 9, 6, 5.	S.—8, 7, 4.
D.—10, 7.	D.—8, 6, 2.
C.—Ace, Kg.	C.—10, 7, 5, 4.

REMARKS.—At trick 7 B argues that he cannot win the game unless he and A can make two tricks in clubs, or unless A can bring in the diamonds. Neither result is at all probable unless A holds an honour in clubs, and in that case it is clearly better that B should clear the way by leading his queen. B therefore treats his long suit as if it were a short one, but his tactics are defeated by Y's refusal to draw the trump. It will be found on trial that B would do no better by leading a small club.

Chess Column.

By C. D. LOCOCK, B.A.Oxon.

The Proprietors of this Journal propose to offer a set of Staunton Chessmen and Box, by way of prize for a KNOWLEDGE Chess Problem Tournament, in connection with this Column. The Competition will begin with a problem in the July number. Six problems will decide the result, unless a tie should necessitate one or two more contests. The score will be reckoned by points, as follows:—

Two-move problems:—For correct key-move, 3 points; variations unnecessary.

Three-move problems:—For correct key-move *and second moves*, 8 points. *Two points will be deducted for each second move omitted or incorrect.* Solutions must be sent in by the 10th of each month. Should a problem admit of a second key-move, *two* additional points will be awarded for the discovery in the case of two-move problems, and *four* additional points in the case of three-move problems. *The same number of marks will be deducted should the claim be incorrect.*

Intending competitors are invited to send for insertion *one* problem of their own composition, not previously published.

The composer will score full marks for the solution of his own problem, if inserted; but if other solvers should find a second solution, he will score *none*.

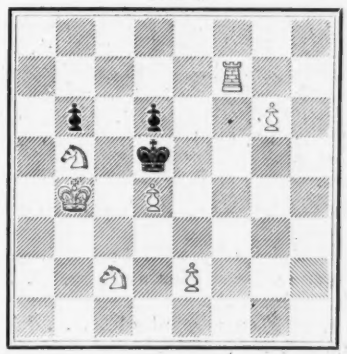
TO CORRESPONDENTS:—*C. T. Blanshard.* Q to R6 will not solve Mr. Mortimer's problem. After 1. . . . Kt to K3, 2. Kt to Kt6, K×Kt, there is no mate. Also after 1. . . . Kt to Ksq, 2. Q to B8, Black saves the mate by 2. . . . Kt to Q3!, not to mention the check with the Rook.

A. J. Luisham. If 1. Q to R6, Kt to K3; 2. Q×Kt, R to K2!, pinning the Queen.

SOLUTION OF PROBLEM (*By J. Mortimer*). 1. Q to R7, Kt to K3, 2. Kt to Kt6, etc. The other variations are obvious. This is, strange to say, the only problem which Mr. Mortimer has composed.

PROBLEM.

BLACK.



WHITE.

White to play, and mate in two moves.
(A very easy practice problem.)

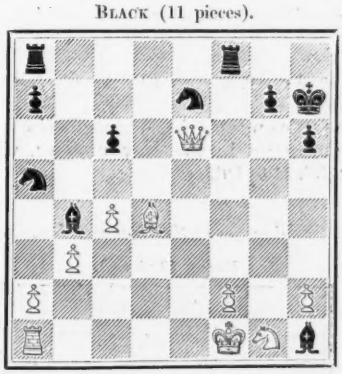
CABLE MATCH.

Below is given the remainder of our analysis of the "Two Knights" Game.

Position after Black's 27th Move.

M. TSCHIGORIN.

BLACK (11 pieces).



WHITE (10 pieces).

W. STEINITZ.

WHITE. (Steinitz.)	BLACK. (Tschigorin.)
28. Q to R3 (*s*)	28. Kt to B4
29. B to K5 (*t*)	29. QR to Ksq
30. B to B4 (*u*)	30. Kt to Q5 (*v*)
31. Q to Q3ch (*w*)	31. B to K5
32. Q×Kt (*x*)	32. R×B
33. P to B3 (*y*)	33. QR to KBsq
34. Q×RP	34. P to B4! (*z*)
35. Q to QB7 (1)	35. Kt to B3
36. P to QR3	36. R×Pch (2)
37. Kt×R	37. R×Ktch
38. K to Ktsq	38. BQ7
39. Resigns (3)	

NOTES.

(*s*) Mr. Steinitz rightly decides to act strictly on the defensive. The object of this move is chiefly to defend the QKtP in case he gets time for P to QR3. It also allows a check at Q3, in case of need. He dare not play immediately 28. P to QR3 on account of 28. . . . KtB4!; 29. BKt2 (after 29. P×B, Kt×B, Black wins easily); 29. . . . QRKsq; 30. QQ7, KtK6 ch; 31. KK2, KtQ8! dis. ch. mating or winning the Queen.

(*t*) 29. BKt2, QRKsq, would leave him no defence. Nor could he allow the Bishop to be exchanged at K3.

(*u*) Obviously, if 30. PKB4, R×B, 31. P×R, KtKt6 mate. 30. BKt3 would also lose speedily.

(*v*) Preventing KtK2. After 30. . . . RK5; 31. KtK2, KtQ5; 32. Kt×Kt, QR×B; 33. KtK6, R×P ch; 34. KKtsq, KRB6; 35. QR4, Black has to waste time in freeing his Bishop.

(*w*) For QKt3 see note (*x*). If 31. B×P, P×B; 32. QQ7 ch, RK2; 33. Q×Kt, RKKtsq wins, as pointed out in the *Daily News*.

(*x*) 32. QKt3 loses by PKKt4; 33. BBsq! (otherwise KtB4 followed by PKt5 wins at once), 33. . . . KtB7 ch winning the exchange.

(*y*) Not Q×RP on account of . . . . RKt5, 34. PB3, BQ6 ch, and 35. . . . RKt4.

(*z*) Shutting out the Queen for the remainder of the game. Mr. Tschigorin judiciously reserves the capture of the KBP. After the exchange of pieces Black has only one check, for the Queen could sacrifice herself for two Rooks.

(1) 35. PQR3 is useless on account of KtB3 (not Kt×KtP; 36. P×B, R QRsq.; 37. Q×R, etc.)

(2) A beautiful and unexpected move, which forces a win in a few moves.

(3) QKt6 is the only move to save the Queen. Black would then mate in six moves.

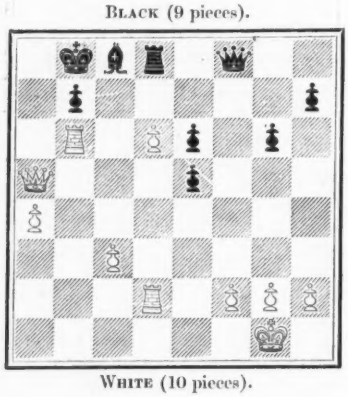
Mr. Tschigorin's play throughout could hardly be improved on.

The Evans Gambit was also resigned by Mr. Steinitz at the same time.

Only one move on each side was made since the publication of the diagram in the May number, viz., 36. . . . K to Ktsq.; 37. P to Q6.

Diagram of the final position.

BLACK (9 pieces).



WHITE (10 pieces).

Black resigns.

[For if 37. . . . Q to B5; 38. R×P ch, B×R (K×R leads to the loss of the Queen in four more moves); 39. Q×R ch, K to R2; 40. Q to R5 ch, BR3 (otherwise the Pawn goes to Queen); 41. Q to B5 ch; and 42. Q to K3, winning easily.]

Mr. Steinitz has put his two most recent eccentricities to a crucial test, with an unsatisfactory result. The two defences are not only purposeless, but, as pointed out in the *Chess Monthly*, inconsistent with each other. In the one case Mr. Steinitz moves a piece to a bad square in order to be attacked; in the other case he does the same thing in order *not* to have the piece attacked. In each case the defence is made more difficult than is necessary.

Theory of the Chess Openings. By G. H. D. GOSSIP. (Messrs. W. H. Allen & Co.) This, the author's third treatise on the subject, is the latest contribution to English chess literature. The book is of prepossessing exterior, the brilliant colour of the binding being rendered still more brilliant by a diagram of what is variously described in the introduction as "Mr. Gossip's historically magnificent performance," or, more briefly (but in larger type), GOSSIP'S BRILLIANT MATE. [This is a position taken from a game in the American Tournament of 1889, which, in Mr. Gossip's opinion, should have taken the brilliancy prize.] The author has adopted, for the first time, the column arrangement of variations. The experiment is not altogether a success: the columns appear to have been arranged regardless of natural sequence, and in at least one case have been repeated word for word. The analytical portion of the work is satisfactory on the whole. Perhaps Mr. Steinitz has been too unquestioningly followed. The author might certainly "venture to differ" more often than he does. The work is mainly, as it professes to be, a compilation from all the modern sources, but much of it is the author's own. Especially noticeable is the adequate treatment of the Vienna opening, several branches of which have been hitherto strangely neglected. The book teems with personal controversy, more entertaining than instructive, vindication of capability being the main topic; but, judged simply from a chess point of view, it is of undeniable value, being exhaustive and thoroughly up to date. The price is extremely moderate, and the printing and binding excellent.

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